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Bérengère Patault. Firms' growth in frictional markets. Economies et finances. Institut Polytechnique de Paris, 2021. Français. NNT : 2021IPPAX039 . tel-03373201

HAL Id: tel-03373201

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INSTITUT
POLYTECHNIQUE
DE PARIS

NNT : 2021IPPAX039

Thèse de doctorat



Firms' growth in frictional markets

Thèse de doctorat de l'Institut Polytechnique de Paris
préparée à l'École Polytechnique

École doctorale n°626 École Doctorale de l'Institut Polytechnique de
Paris (ED IP Paris)
Spécialité de doctorat: Sciences économiques

Thèse présentée et soutenue à Palaiseau, le 23 juin 2021, par

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Acknowledgements

Je tiens tout d'abord à remercier mon directeur de thèse Francis Kramarz. Après mon Master en économie, j'ai eu la chance d'être assistante de recherche pour Francis pendant un an. Cette année a été cruciale et m'a décidé à poursuivre une thèse sous sa direction. Je ne pourrai jamais assez remercier Francis pour son soutien tout au long de la thèse, pour les discussions de recherche inspirantes, ainsi que pour sa disponibilité. Son inspiration débordante et sa passion m'ont toujours motivée et ont su me donner le goût de la recherche.

Je tiens également à dire un immense merci à Isabelle Méjean. Son aide précieuse, ses relectures ou son soutien lors de ma recherche de poste ont été capitaux. Isabelle a dédié beaucoup de son temps pour m'aider, et je suis heureuse de la compter aujourd'hui parmi les membres de mon jury de thèse.

I cannot be thankful enough for Marianne Bertrand and Ferdinando Monte, who accepted to be referees for this thesis, and whose work I have always admired. I would also like to warmly thank Luca David Opromolla, who accepted to be one of the examiners of this thesis.

I would like to express my sincere gratitude to Sam Kortum, who invited me to Yale University during Fall 2019, and who accepted to be part of my jury. My visiting experience at Yale was incredible and had a decisive impact on my motivation to investigate ambitious research questions. I had the pleasure to have many insightful discussions with Sam during my visiting, and I have learned a lot on theoretical models of international trade by his side.

Je suis très reconnaissante envers Pierre Cahuc, qui a encadré mon mémoire de Master, et avec qui j'ai continué à travailler pendant ma thèse sur le thème des indemnités de licenciement. Pierre a été le premier à m'intéresser à la recherche, et j'ai immensément appris en écrivant un article de recherche avec lui.

Je remercie chaleureusement mes (autres) co-auteurs Stéphane Carcillo, Clémence Lenoir, Flavien Moreau et Antoine Valtat. J'apprends tous les jours un peu plus à leurs côtés. Ecrire des articles avec des chercheurs aussi curieux, talentueux et ouverts d'esprit a vraiment transformé mon expérience de doctorante.

I thank all the Professors and PhD students I have had the chance to meet at Yale Univer-

sity, during my visiting of Fall 2019. I also am indebted toward the many (virtual) encounters during my job market. I received valuable feedback on my work and was very happy to meet so many different researchers.

Je souhaite remercier l'ensemble des professeur(e)s du laboratoire, toujours bienveillants et disponibles, notamment Marie-Laure Allain, Geoffrey Barrows, Christophe Bellégo, Christian Belzil, David Benatia, Pierre Boyer, Claire Chambolle, Béatrice Cherrier, Philippe Choné, Julien Combe, Gregory Corcos, Bruno Crepon, Patricia Crifo, Laurent Davezies, Xavier d'Haultfoeuille, Roxana Fernandez, Bertrand Garbinti, Olivier Gossner, Guillaume Hollard, Anett John, Yukio Koriyama, Laurent Linnemer, Olivier Loisel, Franck Malherbet, Guy Meunier, Pierre Picard, Julien Prat, Florence Puech, Alessandro Riboni, Pauline Rossi, Linda Schilling, Benoit Schmutz, Anthony Strittmatter, Emmanuelle Taugourdeau, Thibaud Vergé, Michael Visser, pour leurs conseils avisés et leur soutien. Je remercie particulièrement Arne Uhlendorff, membre de mon comité de suivi pendant ces quatre ans, ainsi que Grégory Corcos, dont les avis répétés sur mon job market paper ont toujours été pertinents et précieux.

Je n'aurais pas pu réaliser cette thèse sans l'aide des personnels administratifs du CREST qui apportent un soutien logistique et financier aux doctorants lors des conférences et visiting. Je pense à Pascale Deniau, Murielle Jules, Weronika Leduc, Eliane Madelaine, Lyza Racon, Arnaud Richet, Fanda Traore et Edith Verger.

I thank the Postdoctoral students who have spent some time at Crest during my PhD, and particularly Horng Chern Wong and Jay Euijung Lee, who helped me during my job market year.

Je tiens à remercier l'ensemble des doctorants croisés au Crest. Malheureusement, l'espace et le temps me manquent pour individualiser chacun de mes remerciements. Pour leur soutien et les rigolades au quotidien, je pense en particulier à mes co-bureaux Héloïse Cloléry, Germain Gauthier, Lucas Girard et Anasuya Raj. Pour leurs conseils avisés et leur soutien psychologique dans les moments difficiles je remercie Antoine Bertheau, Pauline Carry, Antoine Ferey et Clémence Lenoir. Pour les mythiques Magnan je remercie Rémi Avignon, Gwen-Jiro Clochard, Jules Depersin, Etienne Guigue, Yannick Guyonvarch, Elia Pérennès et Fabien Perez.

Toutes générations confondues, je remercie également Reda Aboutajdine, Hélène Benghalem, Marianne Bléhaut, Guidogiorgio Bodrato, Léa Bou Sleiman, Arthur Cazaubiel, Pierre-

Edouard Collignon, Jeanne Commault, Morgane Cure, Thomas Delemotte, Christophe Gail-
lac, Germain Gauthier, Morgane Guignard, Malka Guillot, Jeremy Hervelin, Morgane Hoff-
mann, Myriam Kassoul, Margarita Kirneva, Raphaël Lafrogne-Joussier, Alice Lapeyre, Alexis
Larousse, Claire Leroy, Alexis Louaas, Victor Lyonnet, Esther Mbih, Denys Medee, Alfonso
Montes-Sanchez, Ninon Moreau-Kastler, Alicia Marguerie, Martin Mugnier, Sandra Nevoux,
Elio Nimier-David, Ivan Ouss, Louis-Daniel Pape, Félix Pasquier, Julie Pernaudet, Audrey
Rain, Mathilde Sage, Emilie Sartre, Daphné Skandalis, Clémence Tricaud, Jérôme Trinh, An-
toine Valtat, Benjamin Walter et Ao Wang.

Je tiens enfin à remercier mes amis et ma famille pour leur soutien infailible, dans les
bons comme les mauvais moments. J'ai hâte de pouvoir lire cette thèse aux petits Auguste et
Gaspard. Pour finir, merci à Alexandre d'avoir été là pendant toutes ces années si importantes.
Merci.

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General Introduction

The unifying theme of this thesis is firms' growth, in both international and domestic markets. Understanding how firms grow is crucial to understand aggregate outcomes and fluctuations, such as GDP or a country's export dynamics. In this general introduction, I first review the evidence on the role of firms in shaping such aggregate outcomes, in particular through trade and their impact on the labor market. I then discuss the frictions faced by firms and which slow down firms' growth. Finally, I briefly discuss how each of the three chapters of this dissertation contributes to understanding firms' growth in frictional markets.

1. The role of firms for aggregate outcomes

1.1. Firms in International Trade

“Why do countries trade with each other? How to explain which country trades with which country?” These questions have long constituted the heart of international trade studies. International trade has been rationalized through the principle of comparative advantage, which stems from either technology differences (see Ricardo (1891), Dornbusch, Fischer and Samuelson (1977) and Eaton and Kortum (2002)) or differential factor endowments across countries (see Heckscher (1919)). While such models - known as “Old Trade Theory” - can explain inter-industry trade, they cannot explain why similar trading partners exchange goods within the same industry. For example, they cannot help rationalize why Germany sells cars to the US, while, at the same time, the US sell cars to Germany. In response to this shortcoming, the 1980s saw the appearance of “new” trade models (see Krugman (1980) and Helpman (1981)) in which the key feature is economies of scale. Such economies of scale make trade even more beneficial and give incentives for countries to specialize.

These trade models - be them “old” or “new” - abstract from one key aspect: trade is not performed by countries but by individual firms and plants, which are very heterogeneous.

The growing access to firm-level trade data has enabled to uncover robust patterns on exporting firms in many developed countries such as the US or European countries. In particular, researchers have shown that: i) only a small fraction of firms exports, ii) exporting firms are much larger on average than non-exporting firms, and iii) a country's total exports are highly concentrated within a few exporting firms.

First, the share of firms which export is small. In the US for instance, only 18% of the manufacturing firms are exporters.¹

Second, firms that export are very different from firms that do not, even before they start exporting. This 'export premium' is visible along several dimensions: exporting firms are larger, more productive, and pay higher wages. In the US for instance, exporting firms have 97 percent more employment than non-exporting firms (see Bernard et al. (2007)). Because this difference exists even before the export event, this means that it is driven by a self-selection of firms into exporting. To explain this selection into exporting, Melitz (2003) puts forth a theoretical model with heterogeneous firms along the productivity dimension. In this model, exporting requires firms to pay a fixed cost that only the most productive firms can incur. Taking into account the self-selection of firms into exporting provides another source of welfare gains brought by a reduction of trade costs: the reallocation of market shares toward the most productive firms of a given industry. Third, international trade is concentrated within a few firms. In the US, 1% of firms are responsible for 80% of total exports (see Bernard et al. (2007)). This large concentration implies that a few firms can partly determine the economic performance of a whole country. The importance of a few individual firms has been coined in the literature as "granularity": such granular firms have an impact on aggregate outcomes and aggregate fluctuations (see Gabaix (2011), Di Giovanni, Levchenko and Mejean (2014) and Di Giovanni, Levchenko and Mejean (2018)).

The most recent advance of international trade studies consists in going even further in the level of analysis and to shift from "Which firms trade?" to "Which firm trades with whom?". This has been made possible thanks to access to firm-to-firm trade data, which record the firm identifier of both the exporting and importing firms (see Wagner (2016) for a survey on the use of such data). This novel data enable to decompose trade into several margins at the firm level: how much does a firm sell to a given firm? How many products does a firm export? How many countries does a firm sell to? How many buyers does a firm sell to? The last margin - called the "buyer margin" - has been found to be one of the most important determinants of the total value of exports of a firm (see Bernard, Jensen, Redding and Schott (2009), Bernard,

¹Source: Bernard, Jensen, Redding and Schott (2007), figure from the 2002 U.S. Census of Manufactures.

Bøler and Dhingra (2018b), Bernard, Dhyne, Magerman, Manova and Moxnes (2019b) and Carballo, Ottaviano and Martincus (2018)). The bulk of exporters have a small number of buyers, whereas a very small number of exporters have many buyers. In France's average export market, 65% of exporters interact with a single buyer, and 90% with at most five buyers.² Similarly, most importers have a few French suppliers, and a few buyers have many suppliers. This means that heterogeneity on the product market is two-sided, *i.e.* concerns both the supply and the demand side. Understanding why firms buy from some firms rather than others thus matters to understand trade flows across countries (see Eaton, Kortum and Kramarz (2016)). In the first chapter of my thesis, I show that managers' mobility between exporting firms is key for the matching of international buyers and producers.

Taking into account the granularity of both selling firms and buying firms is important to understand patterns of international trade, but also to understand labor market outcomes. The existence of very large firms generates some labor market power that firms can exploit to reduce wages below the marginal productivity of labor (see Berger, Herkenhoff and Mongey (2020) and Jarosch, Nimczik and Sorkin (2019)). This echoes long-standing issues in labor economics on the determination of wages and employment, that I review in the next subsection.

1.2. Firms in Labor Economics

Broad questions asked by labor economists range from “Why are some workers unemployed? How can we explain the evolution of total employment and unemployment over time?” to “Why are some workers paid more than others?”. One way to answer these questions is to explore the role of firms in shaping wages and employment, both across workers and over time. Micro-economic data at the firm level indeed reveal substantial dispersion in firm size, productivity, and the average wage paid. Understanding why different types of firms set different wages for similar workers or how different types of firms adapt their employment to react to macroeconomic shocks is key to understanding wage and employment patterns.

Wages It has long been recognized that some employers pay workers with similar skills more than others (see Slichter (1950), Krueger and Summers (1988) and Van Reenen (1996)). The access to matched employer-employee data, since the 1990s, has enabled researchers to investigate this finding further by decomposing workers' wages into a worker component and

²See Lenoir (2019) and Bergounhon, Lenoir and Mejean (2018) for detailed descriptions of French trade patterns.

a firm component. In a seminal paper, Abowd, Kramarz and Margolis (1999) find that firm effects explain a sizeable share of the wage differences between similar workers. More recently, Card, Cardoso, Heining and Kline (2018) survey the literature and find that, on average, firm effects contribute 20% of the overall variance of log-earnings. This result of significant firm effects has led to the development of labor market models with frictions (see Mortensen (2003)), in which the law of one price does not apply. The debate on the importance of firm effects is still ongoing though, as several researchers have developed new methods to disentangle the impact of firms from unobserved workers heterogeneity (see Bonhomme, Lamadon and Manresa (2019) and Bonhomme, Holzheu, Lamadon, Manresa, Mogstad and Setzler (2020)). Numerous studies have also described the role of firms in the rise in earnings inequality (see Card, Heining and Kline (2013) and Song, Price, Guvenen, Bloom and Von Wachter (2019)) or in the gender wage gap (see Card, Cardoso and Kline (2016)).

Employment fluctuations Going beyond wages, a key question in labor economics is how firm shape the level and the cyclicity of employment. Recent research has shown that firms play an important role in employment fluctuations across the business cycle, a role which has been reviewed by Lentz and Mortensen (2010) and Moscarini and Postel-Vinay (2018). Poaching by high-paying firms from low-paying firms is one core contributor of aggregate reallocation and individual wage growth, but this mechanism slows down during recessions. Firms thus contribute to the determination of employment fluctuations. Which employers contribute the most to job creation in good times and in bad times? Moscarini and Postel-Vinay (2012) show that large firms destroy proportionally more jobs relative to small firms in periods of high unemployment. Bertheau, Bunzel and Vejlin (2020) complete this picture and find that low-productivity firms shed proportionally more jobs in recessions as compared to high-productivity firms.

2. The role of frictions for firm dynamics

As reviewed in the previous section, there is plenty evidence on the role of firms in shaping aggregate outcomes, both on the product and the labor market. Understanding how firms grow and how they decline is thus crucial to understand such outcomes. However, in the real world, some frictions prevent firms from growing to their optimal size. I review below the main frictions in the product and labor markets that can hamper firms' growth.

2.1. Frictions in the product market

Product markets are characterized by matching between firms: firms need to find buyers to sell their products to. However, there exist some frictions that slow down the matching process of firms, and thus the formation of firm-to-firm networks. The literature usually classifies these frictions into two types: information frictions and contractual frictions.

Information frictions These frictions arise when buyers and sellers do not have full information on the market. On the one hand, sellers may not have identified the potential buyers for their goods or may not have precise information on the market they desire to sell to (see Allen (2014) and Alborno, Pardo, Corcos and Ornelas (2012)). On the other hand, buyers may meet only a fraction of the potential sellers (see Eaton et al. (2016) and Lenoir, Martin and Mejean (2019)).

Rauch (1999) was one of the first to provide indirect evidence on the role of informational barriers in international trade. He classifies goods into two categories: homogeneous and differentiated, and shows that trade in differentiated goods is more sensitive to geographic distance than trade in homogeneous goods. Subsequently, international trade economists have derived several ways to theoretically model information frictions. In Allen (2014) for instance, firms have to go through a costly search process to acquire information about market conditions. In Chaney (2014), exporters face information frictions about potential buyers: they can meet potential buyers either randomly, or through meeting buyers in the neighborhood of their current buyers. Eaton et al. (2016) develop a theoretical model of matching between buyers and sellers and derive predictions on both firm-to-firm trade and aggregate trade patterns. In such a model, buyers meet sellers at a given meeting rate, which is the inverse of frictions between two countries. Information frictions, through changing the equilibrium quantities and prices of traded goods at the firm level, then have an impact on aggregate trade outcomes.

Contractual frictions These frictions arise when the shipment of the goods cannot be contracted upon: buyers might pay for goods that are not supplied or that are of unsatisfactory quality. Startz (2016) indeed explains that for sellers, it is not only costly to find out what goods are available but also to make sure that goods get sent. She quantifies the magnitude of search and contracting frictions faced by importers and finds that the welfare gains from eliminating the information frictions would be 16%, while the welfare gains from eliminating contractual frictions would be 9%. Therefore, contractual frictions alone are an important source of distortion.

With such a strong impact on production and welfare, one may wonder how firms can mitigate such frictions. Face-to-face meetings between buyers and sellers have been proved to be effective in forming buyer-seller relationships (see Cristea (2011), Poole (2010) and Startz (2016)). Levers to reduce search and information frictions may also include infrastructure and information technology (see Allen (2014), Bernard, Moxnes and Saito (2019a), Jensen (2007), Startz (2016) and Steinwender (2014)). Another way to mitigate those frictions is labor mobility across firms: recruiting knowledgeable workers from exporting firms helps firms expand their international activity (see Poole (2013), Mion and Opromolla (2014) and Mion, Opromolla and Sforza (2016)). This is the idea I exploit in Chapter 1 of this dissertation: I focus on the knowledge transmitted across firms by sales managers and show that the sales managers' job-to-job transitions alleviate information and contractual frictions between suppliers and buyers.

2.2. Frictions in the labor market

One empirical regularity that economists seek to explain is: “Where does wage heterogeneity, across and within individuals, come from? How to explain wage dispersion, at equilibrium, for workers with similar characteristics?” The introduction of search frictions in theoretical models of the labor market is key to explain such equilibrium wage dispersion. Indeed, workers and employers do not meet instantly and costlessly. Individuals have imperfect information about available jobs and wages, while firms have imperfect information on the quantity and quality of potential job candidates. As a result, individuals need to devote considerable effort to look for work: they face search frictions.

Diamond (1971) derived a theoretical model of random search with wage posting, but arrived at a paradox: there is a unique equilibrium in which all firms post the same wage. In other words, there is no equilibrium wage dispersion. This is called the Diamond paradox. In a seminal paper, Burdett and Mortensen (1998) explain that this paradox breaks down if one allows workers to search for jobs while they are currently employed: their model manages to deliver some wage dispersion at equilibrium.

Postel-Vinay and Robin (2002) and Cahuc, Postel-Vinay and Robin (2006) go further and develop models that are able to explain some stylized facts that previous models could not explain: downward wage mobility between job spells, as well as wage mobility within job spells.

Labor market policies can either increase or decrease the search frictions faced by work-

ers and firms. On the one hand, in most countries, some labor market institutions limit the capacity of agents to set quantities and prices freely. Examples include employment protection legislation - which restricts the ability to dismiss workers at will - and minimum wages. Employment protection legislation, through increasing firing costs for instance, increases the distortions on the labor market, and thus the amount of frictions faced by firms (see Bentolila and Bertola (1990) and Hopenhayn and Rogerson (1993)). On the other hand, some policy instruments have the objective to reduce frictions and stimulate workers' mobility across firms. Such measures include for instance zero-hour contracts, in which the employer does not have to provide any minimum number of working hours to the worker (see Datta, Giupponi and Machin (2019)). Other measures have the objective of improving information for workers and firms, for instance job search assistance measures which guide job seekers in their efforts to find work (see Cahuc, Carcillo and Zylberberg (2014), Crépon, Duflo, Gurgand, Rathelot and Zamora (2013) and Crépon and Van Den Berg (2016) for evaluations of such policies).

Chapters 2 and 3 of this thesis examine the impact of two important labor market policies that increase the frictions faced by firms: dismissal regulation and collective wage bargaining. In particular, I examine the effect of such policies on firm and wage dynamics. In the following section, I provide more details on the three chapters of my dissertation and their contribution to the labor and trade literatures.

3. Summary of Chapters

Chapter 1 of this thesis studies one way for firms to alleviate search frictions on international markets: the recruitment of sales managers. The second strand of my work, which comprises Chapter 2 and Chapter 3, considers the effect of labor market frictions on firms' growth and survival.

3.1. Chapter 1: How valuable are business networks? Evidence from sales managers in international markets

My first chapter "How valuable are business networks? Evidence from sales managers in international markets", joint with Clémence Lenoir, studies one specific driver of firms' growth: customer acquisition. More specifically, we explore the role of sales manager recruitments for acquiring buyers from rival firms. Exploiting highly granular firm-to-firm customs data linked with matched employer-employee data, we provide novel causal evidence that firms

can acquire their rival's buyers by recruiting their sales managers. Having interacted with a buyer in the past multiplies the probability for the new recruit to sell to this buyer in the future by 11 times. We find that such business network transmission comes at the expense of the sales manager's former employer. Yet, the buyer does not fully substitute its former suppliers for its new supplier. As a result, sales managers' job-to-job transitions are not zero-sum and generate economic gains. Our work has policy implications on how to help small firms expand abroad, while also indicating that such policies might come at the expense of incumbent firms. Through highlighting the role of sales managers for customer acquisition, this chapter also provides indirect evidence on the search frictions that prevent firms from growing abroad. Finally, this chapter emphasizes how labor market frictions may impact the matching on the product market: restricting labor mobility impedes the transmission of buyer-specific knowledge across firms.

3.2. Chapter 2: Judge Bias in Labor Courts and Firm Performance

In the second chapter, entitled "Judge Bias in Labor Courts and Firm Performance", co-authored with Pierre Cahuc, Stéphane Carcillo, and Flavien Moreau, we study the impact of a negative shock on firms' growth: labor courts' outcomes. In numerous countries, when an employee is fired and deems her dismissal wrongful, she can file a complaint before labor courts. If the dismissal is considered wrongful, the firm must pay a compensation, to be decided by the judges, to the dismissed employee. In this chapter, we collect new data on labor courts' decisions in France and use the quasi-random assignment of judges to analyze the effect of compensations on firms' outcomes in the years after the judgment. From reduced-form regressions, we find that the judge bias has in fact a significant impact on firm survival, sales and employment, but only for those firms that are very small - 10 employees or less - and low-performing - return on assets below the median. There are no significant effects for the other firms. From instrumental variable regressions, in which the judge bias instruments the compensation for wrongful dismissal, we find that an increase in the amount of compensation of 1% of the payroll of the firm reduces employment growth at three years horizon by 3% for small firms whose return on assets is below the median. Overall, this chapter contributes to the vast literature which analyzes the labor market impact of dismissal costs. Yet, our contribution is to provide causal evidence of the impact of dismissal costs on the growth and survival of small firms, an issue that has been overlooked by the literature so far. From this perspective, it also relates to the corporate finance literature that assesses the effect of exogenous cash flow shocks, positive or negative, on firms (see Blanchard, Lopez-de Silanes

and Shleifer (1994), Giroud and Mueller (2017) and Rauh (2006)).

3.3. Chapter 3: Large firms' collusion in the labor market: Evidence from collective bargaining

In my third chapter, entitled “Large firms' collusion in the labor market: Evidence from collective bargaining”, and co-authored with Antoine Valtat, we highlight how one labor market policy - namely sectoral wage agreements - shapes the firm size distribution. In several European countries, collective wage bargaining takes place at the sectoral level, and such a bargaining system has recently been advanced as a model to copy in the US. In this chapter, we provide a theoretical mechanism through which large firms can use sectoral bargaining to evict small firms from the market: the unrepresentativeness of employers' associations. Because large firms tend to participate more in negotiations than small firms, they have the power to decide the minimum wage which will be binding for all firms of the industry.³ By deliberately negotiating high wages, large firms reduce the number of small firms that can compete with them. We empirically test this prediction using French data. We show that the larger the bargaining firms relative to the other firms of the industry - *i.e.* the lower the federation's representativeness, the higher their incentives to raise wage floors. This chapter relates to the recent literature exploring the rise of both the labor and product market power of firms (see Autor, Dorn, Katz, Patterson and Van Reenen (2020) and Grullon, Larkin and Michaely (2019)).

³This sectoral minimum wage cannot be lower than the national minimum wage.

Introduction Générale

Le thème unificateur de cette thèse est la croissance des entreprises, tant sur les marchés internationaux que nationaux. Il est essentiel de comprendre comment les entreprises se développent pour comprendre les résultats agrégés, tels que le PIB ou la dynamique des exportations d'un pays. Dans cette introduction générale, je passe d'abord en revue les preuves du rôle des entreprises dans la détermination de ces résultats agrégés, en particulier l'impact des entreprises sur le commerce international et sur le marché du travail. Je discute ensuite des frictions auxquelles sont confrontées les entreprises et qui ralentissent leur croissance. Enfin, j'explique brièvement comment chacun des trois chapitres de cette thèse contribue à la compréhension de la croissance des entreprises sur les marchés frictionnels.

1. Le rôle des entreprises pour les résultats agrégés

1.1. Les entreprises et le commerce international

“Pourquoi les pays commercent-ils entre eux ? Comment expliquer quel pays commerce avec quel pays ?” Ces questions ont longtemps constitué le coeur des études sur le commerce international. Le commerce international a été rationalisé par le principe de l'avantage comparatif, qui découle soit de différences technologiques entre les pays (voir Ricardo (1891), Dornbusch et al. (1977) et Eaton and Kortum (2002)), soit de différences de dotations en facteurs entre les pays (voir Heckscher (1919)). Ces modèles peuvent expliquer le commerce inter-industriel mais ne peuvent pas expliquer pourquoi des partenaires commerciaux similaires échangent des biens au sein d'une même industrie. Par exemple, ils ne peuvent pas expliquer pourquoi l'Allemagne vend des voitures aux États-Unis, alors qu'au même moment les États-Unis vendent des voitures à l'Allemagne. En réponse à cette lacune, les années 1980 ont vu l'apparition de “nouveaux” modèles de commerce (voir Krugman (1980) et Helpman (1981)) dont la caractéristique principale est les économies d'échelle. Ces économies d'échelle rendent le commerce encore plus avantageux et incitent les pays à se spécialiser.

Ces modèles de commerce international - qu'ils soient "anciens" ou "nouveaux" - font abstraction d'un aspect essentiel : le commerce n'est pas réalisé par des pays mais par des entreprises individuelles, qui sont très hétérogènes. L'accès croissant aux données de commerce au niveau entreprise a permis de mettre en évidence les régularités empiriques suivantes : i) seule une petite fraction des entreprises exporte, ii) les entreprises exportatrices sont en moyenne beaucoup plus grandes que les entreprises non exportatrices, et iii) les exportations totales d'un pays sont fortement concentrées au sein de quelques entreprises exportatrices. Premièrement, la part des entreprises qui exportent est faible. Aux États-Unis, par exemple, seules 18% des entreprises manufacturières sont exportatrices.⁴ Deuxièmement, les entreprises qui exportent sont très différentes de celles qui ne le font pas, même avant qu'elles ne commencent à exporter. Cette "prime à l'exportation" est visible à plusieurs niveaux : les entreprises exportatrices sont plus grandes, plus productives et versent des salaires plus élevés. Aux États-Unis, par exemple, les entreprises exportatrices ont 97% plus de salariés que les entreprises non exportatrices (voir Bernard et al. (2007)). Comme cette différence entre exportateurs et non-exportateurs existe même avant l'exportation, cela signifie qu'elle est due à une auto-sélection des entreprises à l'exportation. Pour expliquer cette auto-sélection, Melitz (2003) propose un modèle théorique dans lequel chaque entreprise a un niveau de productivité différent. Dans ce modèle, exporter nécessite le paiement d'un coût fixe que seules les entreprises les plus productives peuvent assumer. Cette auto-sélection des entreprises à l'exportation fournit une nouvelle source de gains de bien-être liée à une réduction des coûts commerciaux : la réallocation des parts de marché vers les entreprises les plus productives d'une industrie. Troisièmement, le commerce international est concentré au sein de peu d'entreprises. Aux États-Unis, 1% des entreprises sont responsables de 80% des exportations totales (voir Bernard et al. (2007)). Cette forte concentration implique qu'une poignée d'entreprises peut en partie déterminer les performances économiques de tout un pays. L'importance de quelques entreprises individuelles a été qualifiée de "granularité" dans la littérature : ces entreprises granulaires ont un impact sur les résultats et les fluctuations agrégés (voir Gabaix (2011), Di Giovanni et al. (2014) et Di Giovanni et al. (2018)).

L'avancée la plus récente des études sur le commerce international consiste à aller encore plus loin dans le niveau d'analyse et à passer de "Quelles entreprises commercent ?" à "Quelle entreprise commerce avec qui ?". Cela a été rendu possible grâce à l'accès aux données sur

⁴Source : Bernard et al. (2007), chiffre issu du recensement américain des entreprises manufacturières de 2002 - *U.S. Census of Manufactures*.

le commerce entre entreprises, qui enregistrent pour une transaction commerciale donnée les identifiants à la fois de l'entreprise exportatrice et de l'entreprise importatrice (voir Wagner (2016) pour une étude sur l'utilisation de ces données). Ces données permettent de décomposer le commerce d'une entreprise en plusieurs marges : combien une entreprise vend-elle à une entreprise donnée ? Combien de produits une entreprise exporte-t-elle ? A combien de pays une entreprise vend-elle ? A combien d'acheteurs une entreprise vend-elle ? La dernière marge s'avère être l'une des plus importantes pour la détermination des exportations totales d'une entreprise (voir Bernard et al. (2009), Bernard et al. (2018b), Bernard et al. (2019b) et Carballo et al. (2018)). La majorité des exportateurs ont un petit nombre d'acheteurs, tandis qu'un très petit nombre d'exportateurs ont de nombreux acheteurs. Sur le marché d'exportation moyen de la France, 65% des exportateurs interagissent avec un seul acheteur, et 90% avec au plus cinq acheteurs.⁵ De même, la plupart des importateurs ont quelques fournisseurs français, et quelques acheteurs ont de nombreux fournisseurs. Cela signifie que l'hétérogénéité sur le marché des produits est biface, *i.e.* concerne à la fois l'offre et la demande. Comprendre pourquoi les entreprises achètent à certaines entreprises plutôt qu'à d'autres est donc important pour comprendre les flux commerciaux entre les pays (voir Eaton et al. (2016)). Dans le premier chapitre de ma thèse, je montre que la mobilité des managers entre les entreprises exportatrices est essentielle pour l'appariement acheteurs-producteurs sur les marchés internationaux.

Prendre en compte la granularité des entreprises est ainsi important pour comprendre les structures du commerce international, mais aussi pour comprendre le fonctionnement du marché du travail. L'existence de très grandes entreprises génère un certain pouvoir de marché que les entreprises peuvent exploiter pour réduire les salaires en dessous de la productivité marginale du travail (voir Berger et al. (2020) et Jarosch et al. (2019)). Cela fait écho à des questions de longue date en économie du travail sur la détermination des salaires et de l'emploi, que je passe en revue dans la sous-section suivante.

1.2. Les entreprises et l'économie du travail

Les questions générales posées par les économistes du travail vont de "Pourquoi certains travailleurs sont-ils au chômage ? Comment expliquer l'évolution de l'emploi total et du chômage à travers le temps ?" à "Pourquoi certains travailleurs sont-ils mieux payés que d'autres ?". Une façon de répondre à ces questions est d'explorer le rôle des entreprises dans le façonnement

⁵Voir Lenoir (2019) et Bergounhon et al. (2018) pour une description détaillée de la structure du commerce français.

des salaires et de l'emploi. Les données microéconomiques au niveau entreprise révèlent en effet une dispersion importante de la taille des entreprises, de la productivité et du salaire moyen versé. Comprendre pourquoi différents types d'entreprises fixent des salaires différents pour des travailleurs similaires ou comment différents types d'entreprises adaptent leur emploi pour réagir aux chocs macroéconomiques est essentiel pour comprendre les modèles de salaires et d'emploi.

Salaires On sait depuis longtemps que certains employeurs paient plus que d'autres, à compétence de travailleur donnée (voir Slichter (1950), Krueger and Summers (1988) et Van Reenen (1996)). L'accès à des données appariées employeur-employé, depuis les années 1990, a permis aux chercheurs d'approfondir ce constat en décomposant les salaires des travailleurs en une composante travailleur et une composante entreprise. Dans un article fondateur, Abowd et al. (1999) ont trouvé que les effets entreprise expliquent une part importante des différences de salaire entre travailleurs similaires. Plus récemment, Card et al. (2018) ont passé en revue la littérature et ont trouvé que, en moyenne, les effets entreprise expliquent 20% de la variance globale des revenus. Ce résultat a conduit au développement de modèles de marché du travail avec frictions (voir Mortensen (2003)), dans lesquels la loi du prix unique ne s'applique pas. Le débat sur l'importance des effets entreprise est cependant toujours en cours, car plusieurs chercheurs ont développé de nouvelles méthodes pour démêler l'impact des entreprises de l'hétérogénéité inobservée des travailleurs (voir Bonhomme et al. (2019) et Bonhomme et al. (2020)). De nombreuses études ont également décrit le rôle des entreprises dans l'augmentation des inégalités salariales (voir Card et al. (2013) et Song et al. (2019)) ou dans l'écart salarial entre les genres (voir Card et al. (2016)).

Les fluctuations du chômage Au-delà des salaires, une question clé en économie du travail est de savoir comment les entreprises influencent le niveau et la cyclicité de l'emploi. Des recherches récentes ont montré que les entreprises jouent un rôle important dans les fluctuations de l'emploi à travers le cycle économique, un rôle qui a été examiné par Lentz and Mortensen (2010) et Moscarini and Postel-Vinay (2018). Le débauchage par les entreprises à forte rémunération de salariés travaillant dans des entreprises à faible rémunération est l'un des principaux contributeurs à la réallocation globale et à la croissance des salaires individuels, mais ce mécanisme est ralenti lors des récessions. Les entreprises contribuent donc à la détermination des fluctuations de l'emploi. Quels employeurs contribuent le plus à la création d'emplois en période de prospérité et de récession ? Moscarini and Postel-Vinay (2012) montrent que les grandes entreprises détruisent proportionnellement plus d'emplois par rapport

aux petites entreprises en période de récession. Bertheau et al. (2020) complètent ce tableau et constatent que les entreprises à faible productivité suppriment proportionnellement plus d'emplois en période de récession que les entreprises à forte productivité.

2. Le rôle des frictions pour les dynamiques d'entreprises

Comme nous l'avons vu dans la section précédente, il existe de nombreuses preuves du rôle des entreprises dans l'élaboration des résultats agrégés, tant sur le marché des produits que sur le marché du travail. Il est donc essentiel de comprendre comment les entreprises se développent et comment elles déclinent pour comprendre ces résultats agrégés. Cependant, dans le monde réel, certaines frictions empêchent les entreprises d'atteindre leur taille optimale. Je passe en revue ci-dessous les principales frictions qui existent sur les marchés des produits et sur le marché du travail, et qui peuvent entraver la croissance des entreprises.

2.1. Frictions sur le marché des produits

Les marchés des produits sont caractérisés par l'appariement entre les entreprises : les entreprises doivent trouver d'autres entreprises à qui vendre leurs produits. Cependant, il existe certaines frictions qui ralentissent ce processus d'appariement, et donc la formation de réseaux inter-entreprises. La littérature classe généralement ces frictions en deux types : les frictions informationnelles et les frictions contractuelles.

Frictions informationnelles Ces frictions apparaissent lorsque les acheteurs et les vendeurs ne disposent pas d'informations complètes sur le marché. D'une part, les vendeurs peuvent ne pas avoir identifié les acheteurs potentiels ou ne pas disposer d'informations précises sur le marché sur lequel ils souhaitent vendre (voir Allen (2014) et Albornoz et al. (2012)). D'autre part, les acheteurs peuvent ne rencontrer qu'une fraction des vendeurs potentiels (voir Eaton et al. (2016) et Lenoir et al. (2019)).

Rauch (1999) a été l'un des premiers à fournir des preuves indirectes du rôle des barrières informationnelles pour le commerce international. Il classe les biens en deux catégories : les biens différenciés et les biens homogènes, et montre que le commerce de biens différenciés est plus sensible à la distance géographique que le commerce de biens homogènes. Par la suite, les économistes du commerce international ont trouvé plusieurs façons de modéliser théoriquement les frictions informationnelles. Dans Allen (2014) par exemple, les entreprises doivent passer par un processus de recherche coûteux pour acquérir des informations sur les conditions du

marché. Dans Chaney (2014), les entreprises exportatrices font face à des frictions informationnelles concernant les acheteurs potentiels : elles peuvent rencontrer des acheteurs potentiels soit de manière aléatoire, soit en rencontrant des acheteurs proches géographiquement de leurs acheteurs actuels. Eaton et al. (2016) développent un modèle théorique d'appariement entre acheteurs et vendeurs et en tirent des prédictions à la fois pour les flux commerciaux entre entreprises et pour le commerce international agrégé. Dans un tel modèle, les acheteurs rencontrent les vendeurs à un taux de rencontre donné, qui est l'inverse des frictions entre deux pays. Les frictions informationnelles, en modifiant les quantités et les prix d'équilibre des biens échangés entre entreprises, ont ensuite un impact sur les résultats du commerce global.

Frictions contractuelles Ces frictions apparaissent lorsque l'expédition des marchandises ne peut faire l'objet d'un contrat : les acheteurs peuvent payer pour des marchandises qui ne sont pas fournies ou dont la qualité est insatisfaisante. Startz (2016) explique en effet que pour les vendeurs, il est non seulement coûteux de trouver les biens disponibles, mais aussi de s'assurer que les biens sont envoyés. Elle quantifie l'ampleur des frictions informationnelles et contractuelles auxquelles sont confrontés les importateurs et constate que les gains de bien-être découlant de l'élimination des frictions informationnelles seraient de 16%, tandis que les gains de bien-être découlant de l'élimination des frictions contractuelles seraient de 9%. Par conséquent, les frictions contractuelles constituent à elles seules une source importante de distorsion.

Avec un impact aussi fort sur la production et le bien-être, on peut se demander comment les entreprises peuvent atténuer ces frictions. Les rencontres en personne entre acheteurs et vendeurs s'avèrent être efficaces pour nouer des relations acheteur-vendeur (voir Cristea (2011), Poole (2010) et Startz (2016)). Les leviers permettant de réduire les frictions peuvent également inclure les infrastructures et les technologies de l'information (voir Allen (2014), Bernard et al. (2019a), Jensen (2007), Startz (2016) et Steinwender (2014)). La mobilité de la main-d'oeuvre entre entreprises est un autre moyen d'atténuer ces frictions : le recrutement de salariés travaillant initialement dans des entreprises exportatrices aide les entreprises à développer leur activité internationale (voir Poole (2013), Mion and Opromolla (2014) et Mion et al. (2016)). C'est cette idée que j'exploite dans le premier chapitre de ma thèse : j'étudie les connaissances transmises entre entreprises par les directeurs commerciaux et je montre que les transitions d'un emploi à l'autre de ces derniers atténuent les frictions informationnelles et contractuelles entre fournisseurs et acheteurs.

2.2. Frictions sur le marché du travail

Une régularité empirique que les économistes cherchent à expliquer est la suivante : “D’où vient l’hétérogénéité des salaires, entre et au sein des individus ? Comment expliquer la dispersion des salaires, à l’équilibre, pour des travailleurs ayant des caractéristiques similaires?”. L’introduction de frictions de recherche dans les modèles théoriques du marché du travail est essentielle pour expliquer cette dispersion des salaires à l’équilibre. En effet, les travailleurs et les employeurs ne se rencontrent pas instantanément et sans coût. Les individus ont une information imparfaite sur les emplois et les salaires disponibles, tandis que les entreprises ont une information imparfaite sur la quantité et la qualité des candidats potentiels. Par conséquent, les individus doivent consacrer des efforts considérables à la recherche d’un emploi : ils sont confrontés à des frictions de recherche.

Diamond (1971) a dérivé un modèle théorique de recherche aléatoire mais est arrivé à un paradoxe : à l’équilibre, toutes les entreprises proposent le même salaire. En d’autres termes, il n’y a pas de dispersion des salaires à l’équilibre. C’est ce qu’on appelle le paradoxe de Diamond. Dans un article fondateur, Burdett and Mortensen (1998) expliquent que ce paradoxe disparaît si l’on permet aux travailleurs de rechercher un emploi alors qu’ils sont déjà employés : leur modèle parvient à produire une certaine dispersion des salaires à l’équilibre.

Postel-Vinay and Robin (2002) et Cahuc et al. (2006) vont plus loin et développent des modèles capables d’expliquer certains faits stylisés que les modèles précédents ne pouvaient expliquer : la mobilité salariale à la baisse entre les périodes d’emploi, ainsi que la mobilité salariale pour un travailleur au sein d’une même entreprise au cours du temps.

Les politiques publiques du marché du travail peuvent soit augmenter soit diminuer les frictions de recherche auxquelles sont confrontés les travailleurs et les entreprises. D’une part, dans la plupart des pays, certaines institutions du marché du travail limitent la capacité des agents à fixer librement les quantités et les prix. Il s’agit par exemple de la législation pour la protection de l’emploi - qui limite la possibilité de licencier des travailleurs - et du salaire minimum. La législation pour la protection de l’emploi, en augmentant les coûts de licenciement par exemple, accroît les distorsions sur le marché du travail, et donc les frictions auxquelles sont confrontées les entreprises (voir Bentolila and Bertola (1990) et Hopenhayn and Rogerson (1993)). D’autre part, certains instruments de politique publique ont pour objectif de réduire les frictions et de stimuler la mobilité des travailleurs entre les entreprises. Ces mesures comprennent par exemple les contrats zéro heure, dans lesquels l’employeur n’est pas tenu de faire travailler un nombre minimum d’heures de travail au travailleur (voir Datta et al. (2019)).

D'autres mesures ont pour objectif d'améliorer l'information détenue par les travailleurs et des entreprises, par exemple les mesures d'aide à la recherche d'emploi qui guident les demandeurs d'emploi dans leurs démarches (voir Cahuc et al. (2014), Crépon et al. (2013) et Crépon and Van Den Berg (2016) pour des évaluations de ces politiques).

Les chapitres 2 et 3 de cette thèse examinent l'impact de deux politiques du marché du travail qui augmentent les frictions auxquelles sont confrontées les entreprises : la réglementation des licenciements et la négociation collective des salaires. En particulier, j'examine l'effet de ces politiques sur la dynamique des entreprises et des salaires. Dans la section suivante, je donne plus de détails sur les trois chapitres de ma thèse et leur contribution vis-à-vis de la littérature existante.

3. Résumé substantiel

Le chapitre 1 de cette thèse étudie un moyen pour les entreprises d'atténuer les frictions de recherche sur les marchés internationaux : le recrutement de directeurs commerciaux. Le deuxième volet de mon travail, qui comprend les chapitres 2 et 3, examine l'effet des frictions sur le marché du travail sur la croissance et la survie des entreprises.

3.1. Chapitre 1

Mon premier chapitre "How valuable are business networks ? Evidence from sales managers in international markets", en collaboration avec Clémence Lenoir, étudie un levier spécifique de la croissance des entreprises : l'acquisition de clients. Plus précisément, nous explorons le rôle du recrutement de directeurs commerciaux pour l'acquisition d'acheteurs. En exploitant des données de douanes très granulaires entre entreprises et en les fusionnant à des données appariées employeurs-employés, nous apportons pour la première fois une preuve causale que les entreprises peuvent acquérir les acheteurs de leurs rivaux en recrutant leurs directeurs commerciaux. Pour un directeur commercial, avoir interagi avec un acheteur dans le passé multiplie par 11 la probabilité de vendre à cet acheteur à l'avenir. Cette transmission du réseau d'affaires se fait au détriment de l'ancien employeur du directeur commercial, mais l'acheteur ne substitue pas totalement ses anciens fournisseurs à son nouveau fournisseur. Par conséquent, les transitions d'emploi à emploi des directeurs commerciaux ne constituent pas un jeu à somme nulle mais génèrent des gains économiques. Notre travail a des implications en terme de politiques publiques, notamment sur la façon d'aider les petites entreprises à se

développer à l'étranger, tout en indiquant également que de telles politiques pourraient se faire au détriment des entreprises en place. En soulignant le rôle des directeurs commerciaux dans l'acquisition de clients, ce chapitre fournit aussi des preuves indirectes des frictions de recherche qui empêchent les entreprises de se développer à l'étranger. Ce chapitre met également l'accent sur la façon dont les frictions sur le marché du travail peuvent avoir un impact sur l'appariement sur le marché des produits : la restriction de la mobilité des salariés entrave la transmission des connaissances entre les entreprises.

3.2. Chapitre 2

Dans le deuxième chapitre, intitulé "Judge Bias in Labor Courts and Firm Performance", co-écrit avec Pierre Cahuc, Stéphane Carcillo et Flavien Moreau, nous étudions l'impact d'un choc spécifique sur la croissance des entreprises : les décisions judiciaires par les tribunaux du travail. Dans de nombreux pays, lorsqu'un employé est licencié et qu'il estime que son licenciement est injustifié, il peut porter plainte auprès d'un tribunal du travail. Si le licenciement est considéré injustifié, l'entreprise doit verser une indemnité, déterminée par les juges, à l'employé licencié. Dans ce chapitre, nous collectons des données inédites sur les jugements des tribunaux du travail en France et utilisons l'affectation quasi-aléatoire des juges pour analyser l'effet des indemnités sur les résultats des entreprises dans les années qui suivent le jugement. À partir de régressions en forme réduite, nous trouvons que faire face à un juge pro-travailleur a un impact significatif négatif sur la survie de l'entreprise, les ventes et l'emploi, mais seulement pour les entreprises de très petite taille - 10 employés ou moins - et peu performantes. Il n'y a pas d'effets significatifs pour les autres entreprises. À partir de régressions avec variable instrumentale, dans lesquelles l'indemnité est instrumentée par le biais du juge, nous trouvons que lorsque le montant de l'indemnité augmente à proportion de 1% de la masse salariale de l'entreprise, alors la croissance de l'emploi à 3 ans est réduite de 3% pour les petites entreprises à faible performance. Plus généralement, ce chapitre contribue à la vaste littérature qui analyse l'impact des coûts de licenciement sur le marché du travail. Cependant, notre contribution porte sur l'impact des coûts de licenciement sur la croissance et la survie des petites entreprises, une question qui a été négligée par la littérature jusqu'à présent. De ce point de vue, elle se rattache à la littérature de finance d'entreprise qui évalue l'effet des chocs exogènes de flux de trésorerie, positifs ou négatifs, sur les entreprises (voir Blanchard et al. (1994), Giroud and Mueller (2017) et Rauh (2006)).

3.3. Chapitre 3

Dans mon troisième chapitre, intitulé “Large firms’ collusion in the labor market: Evidence from collective bargaining” et écrit en collaboration avec Antoine Valtat, nous mettons en évidence la manière dont une politique du marché du travail - les accords de branche - façonne la distribution de la taille des entreprises. Dans plusieurs pays européens, les négociations salariales ont lieu au niveau sectoriel, et un tel système de négociation a récemment été présenté comme un modèle à copier aux États-Unis. Nous fournissons un mécanisme théorique par lequel les grandes entreprises peuvent utiliser la négociation sectorielle pour évincer les petites entreprises du marché : la non-représentativité des associations d’employeurs. Étant donné que les grandes entreprises ont davantage tendance à participer aux négociations que les petites entreprises, elles ont le pouvoir de décider des salaires minimums obligatoires dans l’ensemble du secteur. En négociant délibérément des salaires élevés, les grandes entreprises réduisent la capacité des petites entreprises à leur faire concurrence. Nous testons empiriquement cette prédiction en utilisant des données françaises. Nous montrons que moins une association d’employeurs est représentative - *i.e.* plus les entreprises qui négocient sont grandes par rapport aux autres entreprises du secteur - plus les salaires planchers augmentent. Ce chapitre s’inscrit dans la littérature récente qui étudie l’augmentation du pouvoir de marché des entreprises (voir Autor et al. (2020) et Grullon et al. (2019)).

Chapter 1

How valuable are business networks? Evidence from sales managers in international markets

This chapter is co-authored with Clémence Lenoir (CREST-INSEE).¹

Abstract. Finding new buyers is crucial for firms to expand their business. This chapter analyzes the role of labor mobility as a mechanism for acquiring buyers from rival firms. We focus on one type of worker likely to build ties with buyers - sales managers - and one type of buyer - foreign partners. Combining French firm-to-firm trade data with matched employer-employee data, we carry out an event-study analysis that exploits the timing of sales-managers' transitions from one firm to another for identification. We find that recruiting a sales manager increases significantly the probability to start selling to the buyers of her former firm. Having interacted with a buyer in the past multiplies the probability that the new recruit sell to this buyer in the future by a factor of 11. This business-network transmission comes at the expense of the buyer's former suppliers and, in particular, the sales manager's former employer. Yet, business stealing is only partial, which leads to job-to-job transitions not being zero-sum. The

¹I thank our PhD advisors, Francis Kramarz and Isabelle Méjean, for invaluable guidance and support. This chapter has benefited from discussions with Joseph Altonji, Antonin Bergeaud, Antoine Bertheau, Nicholas Bloom, Pierre Cahuc, Lorenzo Caliendo, Pauline Carry, Thomas Chaney, Gregory Corcos, Alonso de Gortari, Antoine Ferey, Etienne Guigue, Samuel Kortum, Claire Lelarge, Kalina Manova, Costas Meghir, Elio Nimier-David, Anasuya Raj, Raffaella Sadun, Farid Toubal, Arne Uhlenborff, John Van Reenen and Yanos Zylberberg. I thank Nicolas Berman, Gregory Corcos and Ina Jakel for their discussions of this chapter at conferences. This work was supported by a public grant overseen by the French National Research Agency (ANR) as part of the 'Investissements d'avenir' program (reference: ANR-10-EQPX-17 - Centre d'accès sécurisé aux données - CASD). This project received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programmes: Grant Agreement: 741467 FIRMNET, and Grant Agreement: 714597 TRADENET.

investigation of mechanisms reveals that sales managers accumulate buyer-specific connections, beyond country- and sector-specific knowledge.

Keywords: Firms dynamics, Firms organization, International Trade, Managers.

JEL classification: F14, F16, M3, L2, L25.

1.1 Introduction

How do firms grow? The recent literature on firm-to-firm production networks underlines the crucial role of meeting new buyers (see Bernard, Moxnes and Ulltveit-Moe (2018a) and Bernard et al. (2019b)). However, finding new buyers is costly due to the considerable search frictions in matching buyers and sellers that characterize goods markets (see Allen (2014), Martin, Méjean and Parenti (2018) and Steinwender (2014)). First, information frictions may prevent agents from identifying market conditions, such as the goods available or the identity of buyers and sellers on the market; second, contracting frictions may prevent them from ensuring that the goods sent are of satisfactory quality or that the money actually gets sent (see Startz (2016)). Both of these frictions are exacerbated in international goods markets. Policy tools, such as trade-credit insurance or match-making services, have been developed to alleviate these frictions all over the world.²

In this context, a previous connection with a buyer might be instrumental in forming a new buyer-seller relationship. This chapter focuses on one type of worker who is likely to build these ties - sales managers - and asks the following questions: Can firms acquire their rivals' buyers by hiring their sales managers? And, if so, do sales-manager transitions merely transfer buyers from one firm to another, or do they create economic value? Observing the transferability of connections allows us to understand better the possible moral hazard induced by soft-capital ownership - connections and information - between a worker and the firm. This transferability also has implications for the gains from enforcing trade secrecy and limiting worker mobility across firms.

We start from a novel empirical observation: firms with more sales managers have a larger number of foreign buyers. Using data on French exporters, we find that a 1% higher share of sales managers in a firm is associated with an 0.6% greater number of buyers per employee.

²Export Promotion Agencies across the world have developed policy tools to help small firms find foreign customers. In the United States, for example, the US Trade and Development Agency (USTDA) has developed programs to introduce small and medium-sized firms to qualified buyers and distributors. In France, Business France provides information on foreign buyers who are looking for French suppliers.

This correlation is robust to controlling for sector and firm productivity. In contrast, this elasticity is halved when considering the share of other types of managers. This suggests that sales managers have specific knowledge that helps firms to accumulate a large customer base. One channel through which sales managers might play a role in building a portfolio of buyers is business-network transmission: sales managers can bring some of their former clients with them when changing firms.

We investigate this channel by creating a unique match between two administrative datasets. We track worker movements across firms thanks to French linked employer-employee data, and observe all non-domestic buyers of French exporters in the European Union through firm-to-firm trade data. In an event study, we exploit the timing of sales-manager recruitment to infer their causal effect on the recruiting firms. Our main outcome of interest is the probability that the recruiting firm start selling to a buyer, before and after recruiting a sales manager from a firm that sells to this particular buyer.

We find that recruiting a sales manager from a firm selling to a specific buyer has a positive significant effect on the probability that the recruiting firm sell to that buyer post-recruitment. This probability rises by 0.23 percentage points in the two years following the recruitment. This level effect is to be compared to the mean probability of selling to a buyer, which is only tiny in the sparse network under study. Having previously engaged with a buyer in her previous employment increases the probability that a sales manager start selling to that buyer in her new firm by a factor of 11.4. The productivity of the recruiting firm is important in business-network transmission: a sales manager brings her former firm's buyers to her new firm only if her current employer is more productive than her previous firm.

What are the consequences of this business-network transmission for the other French suppliers of the buyer? We find that the business-stealing associated with business-network transmissions is only partial: the buyer does not fully substitute its former suppliers for its new supplier. We show that both the total number of French suppliers and the value of imports from France for a given buyer increase after sales-managers' job-to-job transitions. This indicates that job-to-job transitions are not zero-sum. Business-network transmission does increase the probability that the buyer and the sales manager's former employer dissolve their relationship, but again business stealing is only partial. This last result is puzzling, given that 90% of buyers purchase a given 8-digit product, in a given year, from only one French exporter. We propose a potential explanation: sales-managers' job-to-job transitions increase the span of products imported by the buyer from France.

The last part of the chapter explores two potential mechanisms behind business-network transmission. First, sales managers could bring buyers to their new firm simply because they

possess country-product specific knowledge. A sales manager who knows about exporting textile materials to Denmark may be useful in finding any Danish textile importer, irrespective of whether the sales manager had a previous relationship with this importer. We show that country-product knowledge explains only 19% of our main effect, while the buyer-specific component of the sales-manager's knowledge is behind the bulk of our main effect. Second, we delve deeper into the nature of this buyer-specific knowledge. Repeated exchanges between sales managers and buyers may foster personal connections, enabling the sales manager to bring the buyer more easily to her new firm. To proxy for the duration of the relationship between the buyer and the sales manager, we interact the sales-manager's age with the length of the relationship between the buyer and the sales-manager's former employer. We find that older sales-manager recruitments positively affect the probability of selling to a buyer only for sufficiently-long relationships between the buyer and the former employer. One potential explanation is that sales managers who stay for a long time in a firm establish personal connections with the buyers with whom they work.

A challenge for identification is that job-to-job transitions are not exogenous. Sales managers may move across firms because of factors affecting either their origin or their destination firm. We investigate whether such interpretations can explain the buyers and sales managers' simultaneous co-movement to a given firm. We find that these alternative interpretations cannot drive the systematic link we find between sales manager recruitments and new buyer-seller relationships.

First, firms benefiting from a positive demand shock may hire more sales managers than other firms. In this case, we may mistakenly attribute this extra demand to the sales-manager's recruitment. The inclusion of firm \times year fixed effects in our empirical specification excludes any firm-year level shock from driving our results. Equally, a French establishment's death could spur both a flow of sales managers and a flow of buyers to its rivals. We address this concern by removing from our sample firms and establishments that are potentially closing, bought, or absorbed during the period under consideration. Second, the poached and recruiting firms may have market-specific strategies that could induce both the sales manager's and the buyer's transition from one firm to the other. Suppose the sales-manager's initial firm intends to downsize its market in a given country. This may spur the departure of sales managers to its rival firm, while increasing the rival firm's probability of starting to sell to buyers in this country. We show that our results are robust to controlling for shocks at the firm-country-year level, so that they are not driven by market-specific strategies.

Two identification issues remain. First, the recruiting firm may undertake actions simulta-

neously to the sales-manager's recruitment. A firm might target a specific buyer and conduct business meetings with it the same year as the sales-manager's recruitment, which would bias the effect of the sales-manager's recruitment upward. We cannot entirely exclude the existence of such omitted variables. However, we do disaggregate our analysis at the quarterly level and still fail to detect any potential pre-trend, which limits the plausibility of this explanation. Second, the first mover may be the buyer, rather than the sales manager. This reverse timing would change the interpretation of our results: according to this story, the sales manager does not help the buyer and French firm to meet, but is rather required to carry out the transaction. In other words, sales managers may be essential intermediaries for a firm to sell to a buyer, even conditional on the buyer and seller already having established a relationship. By showing that sales managers bring buyers close to their former firm's buyers in the country-product dimension, we provide evidence that sales managers do have an exploratory role.

This chapter makes two contributions. Our first is to quantify the transferability of buyer-seller relationships across firms through sales-managers' job-to-job transitions. Our second is to analyze the consequences of this transferability for the buyer's former suppliers. The work closest to ours is by Bisztray, Koren and Szeidl (2018), Meinen, Parrotta, Sala and Yalcin (2018), Mion et al. (2016) and Mion and Opromolla (2014). These show that managers have country-specific knowledge that they transmit to firms after their job-to-job movements. We differ from this existing literature by providing novel causal evidence that managers hold buyer-specific knowledge. In particular, we show that buyer-specific knowledge is much more effective for creating buyer-seller ties than is country knowledge. We also emphasize the importance of sales managers, as opposed to other types of managers, in transferring buyers across firms. Carrying out the empirical analysis at the buyer rather than the country level also opens up the possibility of answering a whole new range of questions regarding intellectual property and knowledge rivalry.

This chapter is closely connected to the trade literature on the effects of search, information, and trust frictions in production networks. In this literature, the levers to reduce search and information frictions focus on infrastructure and information technology (see Allen (2014), Bernard et al. (2019a), Startz (2016) and Steinwender (2014)). We provide evidence for another strategy adopted by firms to reduce search frictions: hiring well-connected sales managers. We also add to the literature on firms learning about demand (see Berman, Rebeyrol and Vicard (2019)) and learning from neighboring firms in order to export (see Fernandes and Tang (2014), Kamal and Sundaram (2016), Koenig (2009), Koenig, Mayneris and Poncet (2010) and Mayneris and Poncet (2015)) and import (see Bisztray et al. (2018)). We suggest

that sales-manager transitions across firms are a potential channel for such learning spillovers across exporters.

The role of managers in creating and maintaining buyer-seller relationships has been analyzed in the management literature via case studies, as well as experimentally in the economics literature. The management literature holds that when an employee leaves a firm, the probability that this firm dissolve its ties with existing buyers rises. Potential mechanisms include the buyer's uncertainty about the initial firm's ability to continue to meet its needs, or reduced trust between the buyer and the seller (see Briscoe and Rogan (2016), Chondrakis and Sako (2020), Raffiee (2017) and Rogan (2014)). Overall, this strand of the literature has emphasized the crucial role of individual employees - be it top executives or low-ranking managers - in creating and maintaining buyer-seller ties. Most of this work is based on case studies of professional services, such as Accounting, Law and Advertising. This chapter is the first to generalize this approach to the economy as a whole. Moreover, we can estimate the causal effect of employee mobility on creating and dissolving buyer-seller relationships. Our results also echo the nascent literature on managerial networks. Cai and Szeidl (2017) show, via a randomized control trial, that regular business meetings between managers increase firm productivity and foster the development of their customer base. Fafchamps and Quinn (2016) underline the role of managerial networks in information diffusion across firms. These papers take an experimental approach. On the contrary, we appeal to a comprehensive dataset and show that sales-managers' relationship networks help to explain the considerable heterogeneity in French firms' performance and number of buyers.

The remainder of the chapter is organized as follows. Section 1.2 presents the data and spells out our definition of sales managers, and Section 1.3 sets out some new stylized facts unveiling their role in exporter performance. In Section 1.4, we investigate one channel through which sales managers help firms build a buyer portfolio: their business networks. We explore whether, when recruited, they bring some of their former firm's clients with them. Section 1.5 considers the potential welfare effects of sales-manager transitions by looking at the effect of the recruitment on the sales-manager's former employer. Section 1.6 then examines the mechanisms behind business-network transmission. Last, Section 1.7 concludes.

1.2 Data and descriptive statistics

We carry out our empirical analysis using an unusual combination of three French datasets: firm-to-firm export transactions, matched employer-employee data, and firm profit and loss

statements. A unique firm identifier allows us to merge these three. We exclude public-sector firms and focus on the 2005-2017 period.

1.2.1 Firm-to-firm trade data

We first use a detailed firm-to-firm export dataset, provided by French Customs, covering the universe of French firms and their exports to European Union destinations. For each transaction, the dataset records the identity of the exporting firm (its SIREN identifier), the identification number of the importer (an anonymized version of its VAT code), the date of the transaction (month and year), the product category (at the 8-digit level of the combined nomenclature) and the value of the shipment. The data cleaning and product harmonization over time for this data are described in Bergounhon et al. (2018). One issue with this data refers to small exporters whose total exports to the European Union in a given year are below a certain threshold: these exporters are not required to declare the product category of the exported goods. We apply the methods described in Bergounhon et al. (2018) to recover part of the missing product categories.³ Over the 2005-2017 period, our data covers 83,180 firms and 2,274,181 buyers. The buyers outnumber French exporting firms as the latter can have very many distinct buyers in the European Union. In 2012, exporters had an average of 27 buyers, with a median value of 7, as shown in Table 1.1. Another distinct feature of the data is that 90% of foreign buyers import a given 8-digit product in a given year from only one French supplier. This suggests that a new relationship between a French supplier and foreign buyer may often come at the expense of the buyer's former French supplier. Our data covers business-to-business transactions, and not business-to-consumers trade. When referring to buyers or clients, we therefore refer only to firms that import from French firms, and not to households.

1.2.2 Matched employer-employee data

We merge the customs data above to a matched employer-employee dataset - *DADS Postes* - over the 2009-2015 period: this describes every firm for which every French employee worked during the calendar year, the number of hours worked, and the wage and occupation. This is the data that we will use to identify sales-manager transitions, matched to the 2005-2017 firm data. This *DADS Postes* dataset has two advantages in our setting. First, workers

³Transactions with missing nc8 represent less than 1% of aggregate trade, and about 85% of missing nc8 concern one-shot exporters. One-shot exporters are not in the scope of our analysis, as we focus on exporter dynamics and buyer acquisition.

Table 1.1 – Customs data: No. of buyers per firm

	No. of buyers					
	Mean	p10	p25	p50	p75	p90
Full sample	27.2	1	2	7	21	58
1-year maturity	8.3	1	1	2	5	15
2-year maturity	15.5	1	1	3	10	32
3-year maturity	18.0	1	2	4	12	38
5+ maturity	30.5	1	3	8	24	65
8+ maturity	32.2	1	3	9	26	68

Notes: The figures refer to the number of foreign European buyers of French firms in firm-to-firm Customs data over the 2005-2017 period. We define maturity as the number of years for which the French firm has been exporting.

are characterized by a detailed 4-digit occupation code, as well as occupation labels, which together enable us to identify sales managers. Second, workers have individual identifiers, which allows us to track them across firms over time.

The worker’s occupation is coded at the 4-digit level, describing the worker’s function within the firm. We wish to identify workers who likely play a role in the construction of a portfolio of foreign buyers. We define as sales worker as those whose occupation title contains one of the following keywords: ‘advertising’, ‘clientele’, ‘communication’, ‘commercial’, ‘marketing’, ‘merchant’, ‘sales’ or ‘seller’.⁴ We do not want to consider employees with only limited responsibilities, and who thus do not cater to foreign buyers. As such, we focus on sales managers, *i.e.* sales workers who are likely to have a supervisory role within the firm. These are identified by excluding the two lowest levels of the five-level hierarchical occupation classification codes.⁵ We define other managers within the firm as those at the top three hierarchical levels but who are not in the ‘sales’ function.⁶

Table 1.2 shows the different occupational titles of sales managers, along with the number of sales managers in each occupation and their average wages. The main sales occupation in terms of employment is ‘Commercial managers of small and medium-sized enterprises (excluding retail trade)’. Table 1.3 presents the descriptive statistics for sales managers in our 2012 data. 61% of exporters employ at least one sales manager, and sales managers account for 15% of

⁴In French: clientèle, communication, commercial, commerçant, marketing, publicité, vente and vendeur.

⁵The PCS-ESE classification has five hierarchical levels, from 2 to 6. Managers are found in the highest occupational levels 2 to 4: middle-level managers at hierarchical level 4 and high-level managers at hierarchical levels 2 and 3 (these latter correspond respectively to the company director, managers, and highly-qualified positions). Caliendo, Monte and Rossi-Hansberg (2015) define hierarchy 2 as ‘firm owners receiving a wage which includes the CEO or firm director’, hierarchy 3 as ‘senior staff or top management positions which includes chief financial officers, heads of human resources, and logistics and purchasing managers’ and hierarchy 4 as ‘employees at the supervisor level which includes quality control technicians, technical, accounting, and sales supervisors’.

⁶These other managers include production managers, R&D managers, and other types of managers.

employment in these firms. These figures are substantially lower for non-exporters: only 17% of the whole sample of firms have a sales manager, and on average sales managers represent only 6% of employment.

Another important feature of the French employer-employee data is that workers have a unique identifier over two consecutive years, making it possible to observe worker transitions across firms.⁷ However, as we can track workers for only two consecutive years, we cannot reconstruct the whole history of a sales manager's employment. From the viewpoint of the recruiting firm, we define sales-manager recruitment as the recruitment of a worker who was a sales manager in the previous firm.⁸

On average, sales managers tend to move to more-productive, higher-paying and younger firms. Among the sales managers who move across exporting firms, 55% went to a more-productive firm - where we proxy productivity by the value added per worker in the firm - and 55% to a younger firm. This is consistent with Haltiwanger, Hyatt, Kahn and McEntarfer (2018), who find that smaller and younger firms are net poachers from other businesses. The median mover is a 36-year old male who earns 47,659 Euros per year.

We use matched employer-employee data over the 2009-2015 period. Data is available prior to 2009, but does not enable the accurate identification of sales managers.⁹ Our event-study analysis thus combines information on sales-managers' movements over the 2009-2015 period and Customs data for 2005-2017.

1.2.3 Tax data

We merge the customs data with tax data from the *FARE* dataset using the firm identifier. The tax data include both balance-sheet and profit and loss statements, and in particular detailed information on firm expenditure and revenue, as well as the main sector of activity.

⁷We identify these transitions only if the worker is inactive for under two years between the two jobs, as we can follow a worker only for two consecutive years. This is not an issue as we focus on job-to-job movements - *i.e.* poaches - and do not aim to model outflows from inactivity.

⁸We do not impose that the worker transition to a sales-manager position, *i.e.* the worker can move from a sales-manager position in her former firm to a non-sales manager position in the recruiting firm. In practice, such transitions are rare, and the results are virtually unchanged if the sample is restricted to sales manager to sales manager transitions.

⁹The information on occupations is much-less detailed before 2009.

Table 1.2 – The most-frequent occupations of sales managers

Top 10 occupational labels	No. obs	Avg. hourly wage
Commercial managers of small and medium-sized firms	139,169	35.8
Executives in retail trade	85,746	27.7
Other commercial sales professionals	76,540	19.7
Analyst in commercial, economic and financial studies	69,775	38.1
Commercial technicians, salespersons for final consumption	65,019	18.5
Control of the operation of sales outlets	63,256	17.7
Commercial managers of large firms	53,892	38.2
Product managers, commercial buyers and marketing executives	47,300	33.0
Commercial technicians, salespersons for final goods for firms	44,700	21.6
Commercial technicians, salespersons for services to companies	41,603	23.4

Notes: The data comes from 2012 DADS French matched employer-employee information. The sample is not restricted to exporting firms. We define as sales managers those whose occupation title contains one of the following keywords: 'advertising', 'clientele', 'communication', 'commercial', 'marketing', 'merchant', 'sale' or 'seller', and who belong to hierarchical levels 3 and 4 (corresponding to managers).

Table 1.3 – The share of sales managers in employment in French firms

	Mean	p50	SD	N
All firms				
Share of firms with at least one sales manager	17%	0	0.38	1,183,424
Share of managers in firm's employment	27%	0	0.37	1,183,424
Share of sales managers in firm's employment	6%	0	0.19	1,183,424
Exporters to the EU				
Share of firms with at least one sales manager	61%	100%	0.49	47,950
Share of managers in firm's employment	38%	33%	0.29	47,950
Share of sales managers in firm's employment	15%	5%	0.21	47,950

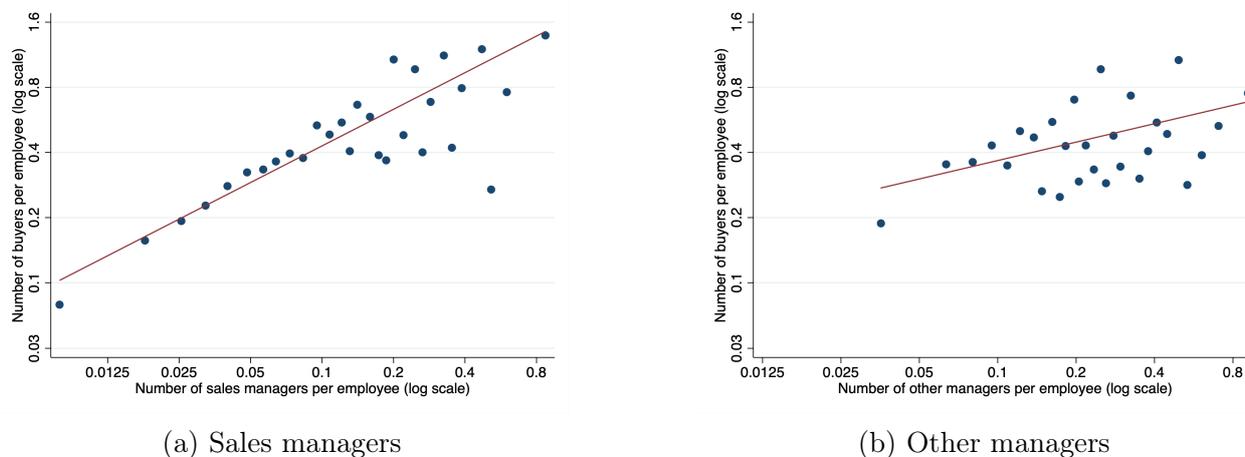
Notes: The figures come from Customs data merged with employer-employee data from 2012. The statistics are at the firm level. The table is read as follows: 17% of all French firms have at least one sales manager, and 61% of exporting French firms have at least one sales manager.

1.3 Stylized facts: Do sales managers play a role in the construction of a portfolio of buyers?

Before exploring the role of sales-manager mobility in acquiring buyers from rivals, we present suggestive evidence that sales managers do help construct buyer portfolios.

It is clear from the data that firms with more sales managers have more foreign buyers, as exhibited in Figure 1.1a for the firm's share of sales managers and the number of buyers per employee. We normalize both variables by firm size in order to avoid picking up a simple size effect. A 1% rise in the share of sales managers in a firm is associated with 0.6% more buyers per employee. In contrast, the regression slope for other types of managers is only half this figure (see Figure 1.1b). While managers overall may help build buyer portfolios, the role of sales managers appears to be more central.

Figure 1.1 – Number of foreign EU buyers per employee and the firm’s share of managers



Notes: Customs data merged with matched employer-employee data 2009-2015. The observations are pooled into bins.

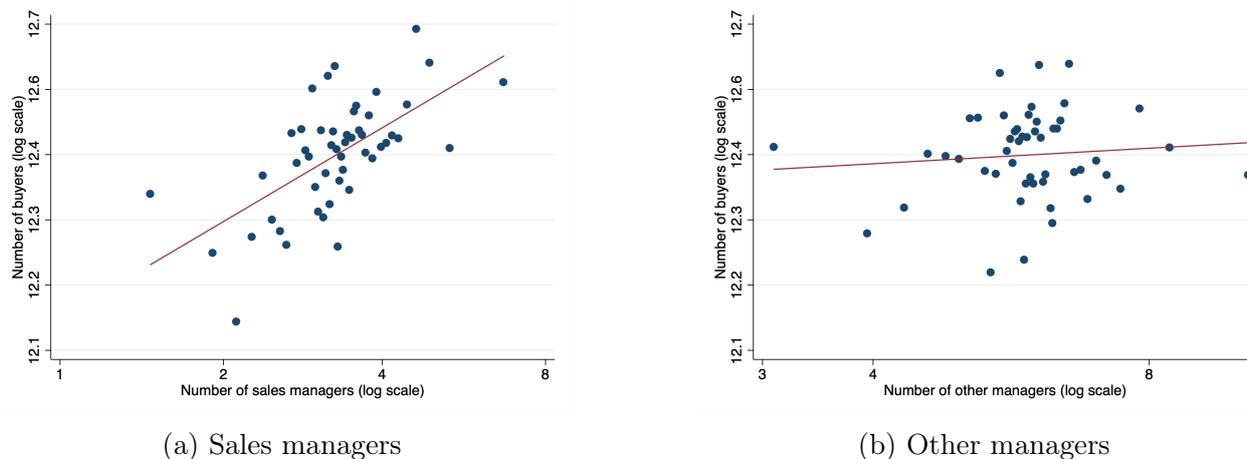
This pattern is robust to including controls such as firm productivity and firm fixed effects. Figure 1.2 shows the correlations between the firm’s number of managers and its number foreign buyers after controlling for log employment, the number of managers, value added and value added per worker. We also remove sectoral and geographical trends, and include firm fixed effects. Figure 1.2a shows that firms employing 1% more sales managers, conditional on their total employment, have 0.03% more foreign buyers. In contrast, we cannot detect any significant association between the employment of other managers and the firm’s number of buyers, as shown in Figure 1.2b. Appendix Table 1.8.3 displays the estimation results.

In Appendix 1.8.3, we investigate the relationship between the number of sales managers in a firm and the other export margins - namely total exports, the number of products exported and the value exported for each product and each buyer. We find that the number of sales managers in a firm is positively correlated with the firm’s total exports and number of exported products, but negatively correlated with the intensive margin - *i.e.* is the value of exports per buyer per product. The role of sales managers is then predominantly to increase the extensive margin of exports.

1.4 Can firms acquire their rivals’ clients by hiring their sales managers?

Conditional on their total employment, firms with more sales managers have more buyers. This suggests that sales managers may help their firms reach new buyers, and so likely have valuable knowledge on how to sell to them. In this Section, we consider the consequences

Figure 1.2 – Number of foreign EU buyers and the firm’s number of managers



Notes: Customs data merged with matched employer-employee data 2009-2015. Controls: log firm employment, log number of managers, log value added and value added per worker, firm fixed effects, sector \times year fixed effects and employment zone \times year fixed effects.

of sales-managers’ job-to-job transitions for buyer-seller relationships. Using an event-study specification, we provide causal evidence that sales managers transfer their business network from firm to firm.

1.4.1 Empirical strategy

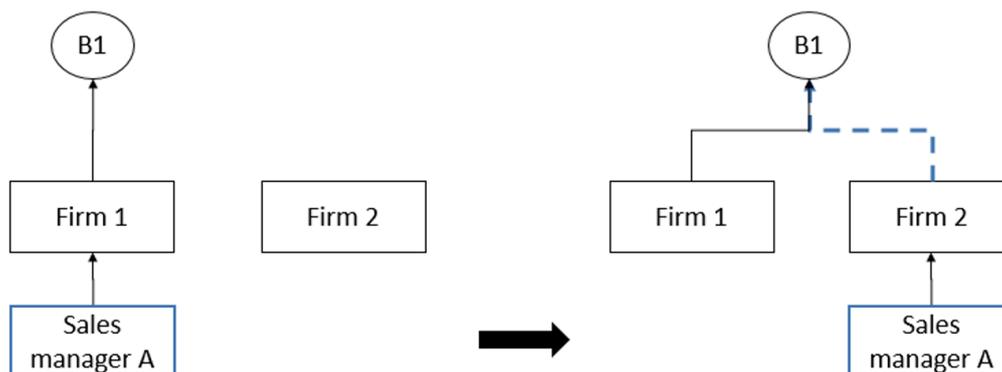
Event-study design If sales managers help their firms find and retain buyers, they may transmit their portfolio of buyers across French firms along their job-to-job movements. To test for this business-network transmission, we estimate the causal effect of a firm recruiting a sales manager on the probability to start exporting to the buyers of the sales manager’s former firm. An example of business-network transmission appears in Figure 1.3. We evaluate whether, on the transition of sales manager A from firm 1, which was selling to buyer B1 in $t - 1$, to firm 2, firm 2 starts to sell to buyer B1. Formally, we study the propensity of a French firm to start selling to a buyer, before and after the recruitment of a sales manager from a firm selling to this particular buyer.

We exploit the timing of sales managers’ recruitments for identification. We assemble a novel dataset at the French firm \times foreign buyer \times year level. Our unit of analysis is the French firm \times foreign buyer, and we define a unit as treated if the French firm recruits a sales manager from a firm selling to that buyer. We select units that are treated exactly once over the [2009,2015] period, and define as controls units that have not yet switched to treatment but will do so in the future. We thus use the staggered adoption of the treatment for identification.

The outcome variable for a French firm \times foreign buyer pair is an indicator variable equal

to one if the French firm starts selling to the buyer, and the treatment variable is a dummy for the firm recruiting a sales manager from a firm selling to that buyer. We then estimate the differential evolution of the probability to start selling to the buyer before and after recruiting the sales manager who is connected to the buyer, for treated versus not-yet-treated units.

Figure 1.3 – An example of business-network transmission



Notes: The left-hand side represents the buyer-seller relationships between French sellers - firm 1 and 2 - and a foreign buyer B1 at $t - 1$. The right-hand side represents the situation at time t . We observe in the data sales manager A moving from firm 1 to firm 2 in period t . Firm 1 was selling to buyer B1 in $t - 1$. We evaluate whether in period t firm 2 starts selling to buyer B1.

Variation exploited A firm might recruit a sales manager after a positive productivity or demand shock. In that case, the greater probability to start selling to a buyer after the sales-manager recruitment may not reflect the sales manager but rather this unobserved shock. Thanks to the high granularity of the data, we can control for these unobserved firm-level shocks via firm \times year fixed effects. Second, firms may recruit from firms that are similar to them in terms of product, sector or geography. It is therefore likely that, even before the sales manager is hired, the firm recruiting the sales manager who knows a particular buyer had a higher probability of exporting to this buyer than another firm. We mitigate this concern by including buyer \times firm fixed effects to control for the average propensity that a French firm and a buyer meet. Third, our sample construction may lead to a certain type of selection bias: we include buyers in our sample who buy from at least one French supplier from which at least one sales manager leaves.¹⁰ As a result, the year of the sales manager's recruitment is likely to correspond to a year in which the buyer faces a positive shock. A simple way of solving this issue is to control for buyer \times year fixed effects, which removes time-variant unobserved shocks at the buyer level. To conclude, after the inclusion of our set of fixed effects, the variation we exploit is *within* a given French firm \times buyer pair *across time*, controlling for both firm-level and buyer-level time-variant shocks.

¹⁰We explain this issue in more detail in Appendix 1.8.5

The baseline equation estimated is as follows:

$$\mathbb{1}(\text{start selling to } b \mid \text{recruiting from firm serving } b)_{fbt} = \sum_{\substack{k=-4 \\ k \neq -1}}^{k=4} \beta_k \mathbb{1}\{t = t_{fb} + k\} + \gamma_{fb} + \gamma_{ft} + \gamma_{bt} + \epsilon_{fbt} \quad (1.1)$$

where t_{fb} is the year of the recruitment by firm f of a sales manager from a firm selling to buyer b and β_k is the effect of recruiting a sales manager on the probability to start selling to buyer b , k years after/before the sales manager's recruitment. γ_{bt} , γ_{ft} and γ_{fb} denote buyer \times year, firm \times year and firm \times buyer fixed effects respectively.

Sample restrictions We restrict the analysis to buyers importing from France continuously from 2005 to 2014. This reduces noise from small buyers who only rarely purchase French imports.¹¹ For each French firm, we also restrict the sample to the potential buyers of the firm's product: a firm mainly exporting jewelry has little probability of selling to a buyer importing plastic plates. Retaining this kind of firm \times buyer pair would introduce noise and increase the dimensionality of our data. The French firm's export products are all the 8-digit level products that it exported at least once over the 2005-2017 period. The same French firm's potential buyers are defined as all the buyers that imported at least once one of the firm's export products at the 8-digit level. We provide more details on the definition of potential buyers following Bergounhon et al. (2018) in Appendix 1.8.5. Last, as our focus is on sales managers who are poached, we restrict our sample of sales-manager movements to those that are voluntary, and so exclude cases where the sales managers have an unemployment - or non-employment - spell before finding a new job. We follow the literature and assume that worker-job spells that are separated by a sufficiently-long period are not job-to-job transitions. In the main analysis we restrict job-to-job transitions to job spells that are under 40 days apart.¹²

1.4.2 Identifying assumption

The key identifying assumption is that of a common trend. This requires that the co-movement of sales managers and buyers toward a given firm should not be related to any factor other than the sales manager's job-to-job transition.

¹¹Buyers who are present every year represent only 6.3% of the total buyers importing from France over the period, but 74.1% of the total export value of French exporters.

¹²Postel-Vinay and Robin (2002) consider that if the duration between two jobs is over 15 days, then the observed transition is not job-to-job but job to non-employment to job. The IAB Job Vacancy Survey, 2000-2013, finds that the average duration between two jobs is 37 days. We thus restrict the duration between two jobs to be under 40 days.

The credibility of this assumption would be stretched without firm \times year fixed effects: positive demand shocks at the firm level could simultaneously spur the recruitment of sales managers and a greater probability of selling to a given buyer. As a result, the probability of starting to sell to a buyer in the absence of recruitment would not exhibit the same time trend across firms. With the fixed effects we include, the identifying assumption requires that there is no shock at the firm \times buyer \times year level that would both shift the probability to sell to a particular buyer and recruit a sales manager connected to this buyer.

This assumption is not testable. However, including leads in our specification enables us to examine the patterns in the data in the years leading up to the recruitment. We calculate these placebo estimators up to $t - 4$, and find no significant effect for any of them. This is reassuring, but does not rule out the existence of shocks at the firm \times buyer \times year level that would be compatible with the absence of pre-trends. We discuss these potential shocks and how we deal with them below.

Poached firms' death The death of a French firm or establishment might spur both a flow out of sales managers and a flow of buyers to its rivals. In this case, we might attribute the increased probability of selling to a former buyer of the dying firm to the sales manager's recruitment. The flows of sales managers and buyers here are simultaneous, and so would not produce significant placebo estimators. In the same vein, the absorption of a firm or establishment by another firm would be classified in our sample as the recruitment of a sales manager. As for the case of firm death, we would see simultaneous sales-manager recruitment and a rise in the number of buyers in the recruiting firm, generating a significant correlation between the two but insignificant placebo estimates. We rule out these mechanisms by removing from our sample firms and establishments that are potentially dying, bought or absorbed during the period (see Picart (2014)). We discuss how we do so in Appendix 1.8.5.

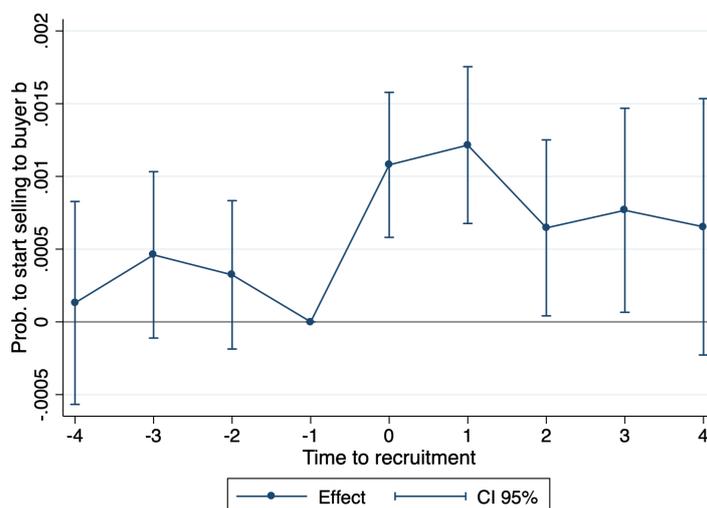
Omitted-variable bias One potential threat to our identification strategy is other actions undertaken by the firm in the same year as the sales manager's recruitment that increase the probability of selling to the specific buyer connected to the sales manager. For instance, marketing targeted at a certain buyer b or business meetings with buyer b may bias upward the effect of the sales-manager network. We cannot entirely exclude this hypothesis and, as a result, annual data may not suffice to spot differential trends across firm-buyer pairs. We thus disaggregate the analysis at the quarterly level, and estimate the outcome for each quarter after the sales manager was hired, as compared to the quarter before the hire. We do not detect any significant effects prior to the recruitment. Were marketing expenditures targeted

at buyer b to drive our results, but not appear in the quarterly pre-trends, they would have to occur in the exact same quarter as the sales manager's recruitment: this overall seems unlikely.

1.4.3 Results

Sales managers who move to new firms bring some of their former firm's customers with them. Figure 1.4 plots the estimated $\hat{\beta}_k$, for $k \in [-4; 4]$: these reveal the effect of recruiting a sales manager from a firm selling to a buyer on the probability to start selling to this same buyer k years before/after the recruitment. This effect is found to be positive and significant in the years following the recruitment, rising by 0.11 percentage points the year of the recruitment. This figure corresponds to a 10.0% increase over the baseline probability (in $t - 1$) of selling to this buyer. This effect appears very rapidly, and then drops off two years after the recruitment. The cumulative effect in the two years after the recruitment is 0.23 percentage points, corresponding to an increase of 21.2% over the baseline probability.

Figure 1.4 – The effect of recruiting a sales manager connected to a buyer on the probability of starting to sell to this buyer

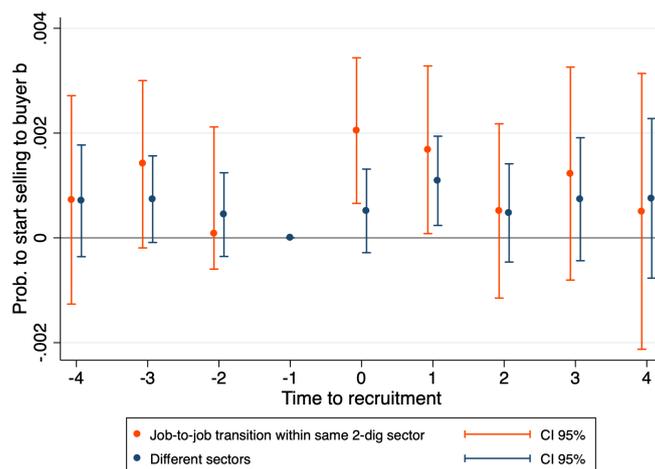


Notes: This graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Equation (1.1), which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b k years before/after the recruitment. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level.

The ability of sales managers to bring their customer network with them likely depends on the sectoral proximity between the previous and new firms. Figure 1.5 depicts the estimated effects for the recruitment of a sales manager between firms in the same 2-digit sector as opposed to a different sector. The effect is much larger in the former case, although we still

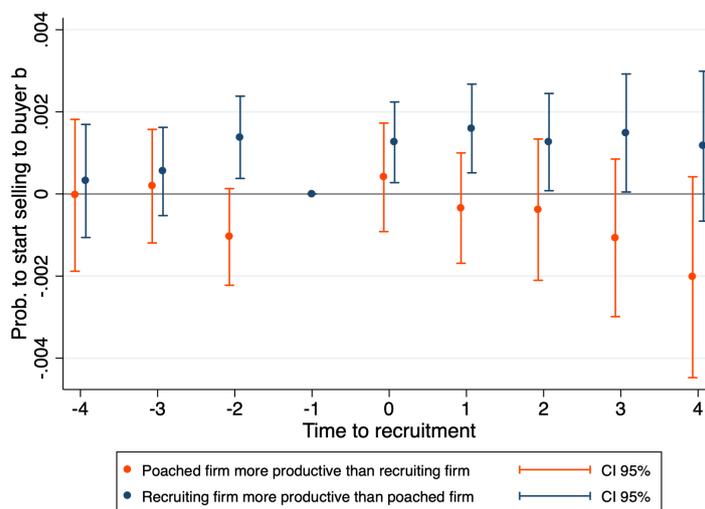
identify a significant effect the year after the recruitment for sales managers who move across sectors. Sales managers may then bring buyers to their new firm even when the previous and current firms sell different products.

Figure 1.5 – The effect of recruiting a sales manager connected to a buyer on the probability of starting to sell to this buyer - same-sector versus different-sector job-to-job transitions



Notes: This graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Equation (1.1), which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b k years before/after the recruitment. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level.

Figure 1.6 – The effect of recruiting a sales manager connected to a buyer on the probability of starting to sell to this buyer - by firm productivity



Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Equation (1.1), which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b k years before/after the recruitment. We measure productivity as the value added per worker. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level.

The productivity of the recruiting firm is an important factor in understanding business-network transmission. Figure 1.6 shows that sales managers bring their former firm’s buyers to

their new firm only when the new firm is more productive than the previous firm. This is consistent with the theoretical literature, which assumes that buyers choose the most productive supplier they meet (see Eaton et al. (2016) and Lenoir et al. (2019)): upon the sales manager's recruitment, the buyer switches suppliers only if the newly-met firm is more productive than the initial one.

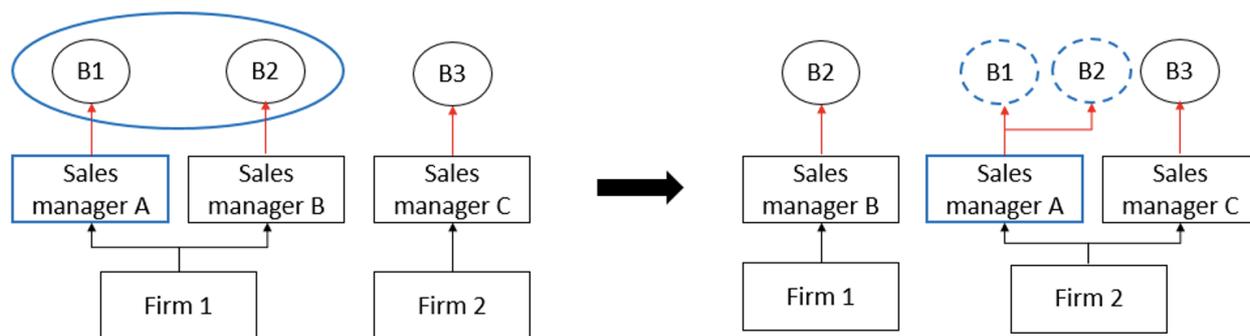
We list a wide array of heterogeneity tests in Appendix 1.8.7. We find, for instance, that the effect is larger for recruiting firms that did not previously export to the buyer's country, small, downstream buyers, and well-paid sales managers.¹³

1.4.4 Economic significance

We have thus far shown that sales managers bring some of their former firm's clients with them when moving jobs. However, we do not observe the ties between sales managers and buyers in the data, and only have a proxy for sales managers' business networks: the buyers of the firm in which the manager previously worked. Figure 1.8 illustrates our empirical setting. We would ideally like to know how the probability to sell to a buyer changes when the new sales manager has previously interacted with the buyer.

Figure 1.7 – An example of business-network transmission

Proxy of sales manager A's network in t-1



Our reduced-form estimation does not answer this question for two reasons. First, the sales manager recruited may not have interacted with the given buyer in her previous firm. In Figure 1.8, sales manager A interacted with buyer B1 in the previous firm 1, but not with buyer B2. Second, the sales manager has a positive probability of meeting a buyer even without any previous interaction, through a random meeting effect. In our example, sales manager A may randomly meet buyer B2 and bring them to firm 2, even without having dealt with B2 before.

¹³See respectively Figure 1.8.18, Figure 1.8.17, Figure 1.8.15 and Figure 1.8.16.

We may thus find a significant effect of recruitment even in the absence of business-network transmission.

Intuitively, the event-study estimand is a weighted average of business-network transmission and other factors that determine any sales manager's propensity to sell to any buyer, including some unobserved random matching:

$$\beta = \omega \times \text{business-network effect} + (1 - \omega) \times \text{random-matching effect}$$

where ω is the probability that the recruited sales manager was the sales manager dealing with buyer b in the previous firm. In order to recover the true business-network effect, we need to identify the weight ω .

Under the assumption that sales managers are identical, each firm's sales managers can be thought of as dealing with the same number of buyers, as in Figure 1.8.¹⁴ We can therefore approximate ω by the inverse of the number of sales managers of the poached firm, which we observe in the data. Interacting these values for ω with dummies indicating the time to treatment produces the following estimate: for a sales manager, having interacted with a particular buyer b increases the probability to sell to that buyer by a factor of 11.4.¹⁵

1.4.5 Robustness tests

We outline below the additional analyses and robustness checks we carry out to alleviate any remaining identification concerns. We show in particular that business-network transmission

¹⁴One obvious limitation of our quantification is that it ignores sales-manager heterogeneity. Some sales managers may be more efficient in meeting buyers than others, and thus may have a larger buyer portfolio. Providing a quantification for our effect here requires assumptions about the functional forms of p_1 and p_2 . We devise a theoretical model in Appendix 1.8.9 that micro-founds particular functional forms. We show that, under reasonable assumptions, our quantification is robust to taking sales-manager heterogeneity into account. In this case, a sales manager having interacted with a particular buyer b increases the probability of selling to that buyer by a factor of 11.8, which is similar to our initial estimate.

¹⁵In this case, the event-study estimand for a recruiting firm with productivity φ and a poached firm with productivity φ' is:

$$\beta(\varphi, \varphi') = \frac{1}{N_s} [p_1(\varphi, \varphi') - p_2(\varphi, \varphi')] + p_2(\varphi, \varphi')$$

where N_s is the number of sales managers in the poached firm, $p_1(\varphi, \varphi')$ the business-network effect and $p_2(\varphi, \varphi')$ the residual term. Substituting $\beta(\varphi, \varphi')$ into our estimation Equation (1.1) gives:

$$\begin{aligned} \mathbb{1}(\text{start selling to } b \mid \text{recruiting from firm serving } b)_{fbt} &= \sum_{\substack{k=-4 \\ k \neq -1}}^{k=4} \mathbb{1}\{t = t_{fb} + k\} \times \frac{1}{N_s} [p_1(\varphi, \varphi') - p_2(\varphi, \varphi')] \\ &\quad + \mathbb{1}\{t = t_{fb} + k\} \times p_2(\varphi, \varphi') + \gamma_{ft} + \gamma_{fb} + \gamma_{bt} + \epsilon_{fbt} \end{aligned}$$

We observe N_s in the data. We can thus regress the outcome variable on both the dummies indicating time to treatment - $\mathbb{1}\{t = t_{fb} + k\}$ - and N_s interacted with these dummies - $\mathbb{1}\{t = t_{fb} + k\} \times \frac{1}{N_s}$ - to recover $\mathbb{E}\{p_1(\varphi, \varphi')\}$ and $\mathbb{E}\{p_2(\varphi, \varphi')\}$. We obtain that $p_1 = \mathbb{E}\{p_1(\varphi, \varphi')\} = 0.0068611$ and $p_2 = \mathbb{E}\{p_2(\varphi, \varphi')\} = 0.0005997$. Dividing p_1 by p_2 answers our initial question: a sales manager having interacted with a particular buyer b increases the probability of selling to that buyer by a factor of 11.4.

is specific to sales managers, and that the effect is robust to using more disaggregated data.

Country-specific strategies The poached and recruiting firms may have market-specific strategies that could affect both the sales manager's and the buyer's transition from one firm to another. Suppose the sales manager's initial firm intends to downsize its market in a given country. This may spur the departure of sales managers to its rival firm, while increasing the rival firm's probability of starting to sell to buyers in this country. We address this issue by controlling for firm \times country \times year fixed effects. Figure 1.8.8 shows that our results are robust, so that firms' market-specific strategies do not lie behind our estimates.

Quarterly level analysis We carry out the event study at the quarterly level. As well as producing finer pre-trend figures, this allows a better understanding of how the timing of the effect unfolds. Figure 1.8.9 displays the results. There is no significant effect of sales-manager recruitment in the quarters before hiring, but as soon as the sales manager is recruited, the probability of starting to sell to the connected buyer rises by 0.053 percentage points. This effect lasts for around six quarters.

Correlated effects One issue we have not entirely ruled out is correlated effects. A large negative shock will simultaneously spur the movement of workers to the firm's competitors and its buyers to its competitors. Here the correlation between the sales-manager's hire and the probability of starting to sell to the sales manager's former buyer stems from correlated effects of the departures of the sales manager and the buyer. This will not be captured by our recruiting firm \times year fixed effects, as it is a time-variant shock at the recruiting firm \times buyer level.¹⁶ However, if this lies behind our results, we should see a similar effect for the hiring of other types of managers. We hence re-estimate our baseline regressions for managers who are not in sales positions. The results appear in Figure 1.8.10a. There is no significant effect of the recruitment of other types of managers. We also separately estimate the effect for production, R&D and other types of managers, and continue to find no significant effects (see Figure 1.8.10b).

Falsification test As a falsification test, we re-run the analysis using placebo years for sales-manager recruitment. For a given firm \times buyer, we assign a random year for the recruitment of the connected sales manager. Figure 1.8.11 displays the results: there is no significant

¹⁶Our sample does not include poached firms that were probably dying (see Section 1.4.2), but we cannot altogether rule out the risk of negative shocks.

impact of sales-manager recruitment on the probability of starting to sell to a connected buyer.

Firms in the same Business Group In France, around half of employees work in firms that belong to what are called business groups. These allow firms to reallocate workers across firms within the same business groups in a more-seamless way, and thus to more-easily adjust to any shocks (see Cestone, Fumagalli, Kramarz and Pica (2018)). Sales managers who move across firms in the same business group may do so in response to negative shocks borne by the origin firm, or positive shocks to the destination firm. We carry out the same event study as in the main specification, but exclude sales managers who move between firms in the same business group. Figure 1.8.12 shows the results, which are qualitatively and quantitatively unchanged.

The estimator of de Chaisemartin and d’Haultfoeuille (2020) Recent work has demonstrated that standard event-study estimators are biased when the treatment effect is heterogeneous across groups or over time (see Borusyak and Jaravel (2017b), de Chaisemartin and d’Haultfoeuille (2020) and Goodman-Bacon (2018)). The two-way fixed-effect regressions identify weighted sums of the average treatment effects in each period and for each group and period. When the treatment effect is heterogeneous, be it across groups or over periods, the weights can be negative, thus producing biased estimates. We evaluate the prevalence of negative weights in the event study with the standard estimator.¹⁷ We estimate the weights on β : 982,452 weights are positive (82%), while 210,161 are negative. The negative weights sum to -0.075: this does not seem to be a significant amount of treatment heterogeneity. As a complementary strategy, we use as a robustness test the estimator proposed by de Chaisemartin and d’Haultfoeuille (2020), described in detail in Appendix 1.8.4. They estimate the treatment effect by comparing units that have just switched to treatment to units not already treated, which method they argue is robust to treatment heterogeneity. Figure 1.8.13 shows that the pattern is qualitatively unchanged when using their estimator or the standard event-study estimator.

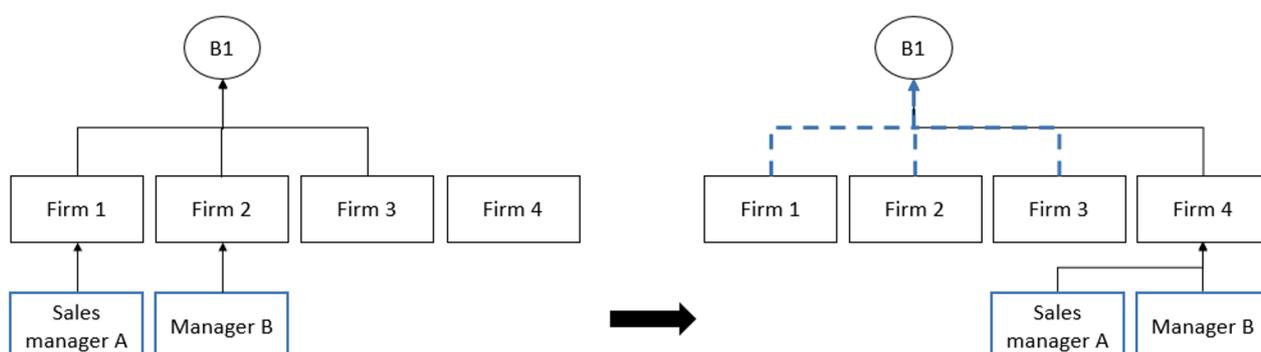
¹⁷We do so using the Stata command ‘`twowayfeweights`’, designed by de Chaisemartin and d’Haultfoeuille (2020).

1.5 Implications: Do sales managers steal their former firm's buyers?

Sales managers bring some of their former firm's buyers to their recruiting firm. We now consider the implications for the sales manager's former employer to see whether sales managers' job-to-job transitions merely transfer buyers from one firm to another or, on the contrary, create economic value.

We here focus on sales managers who bring some of their former firm's buyers to their new firm, to see what happens to the previous French suppliers of this buyer. Suppose that a buyer imported from three French suppliers at the beginning of the period, as in Figure 1.8. In a given year, supplier 1 loses a sales manager to firm 4, supplier 2 loses another type of manager to firm 4, and supplier 3 does not lose any managers to firm 4. We observe that firm 4 starts to sell to the buyer this given year, and ask whether the relationships between the buyer and suppliers 1, 2 and 3 are dissolved. We first show that the buyer does not fully substitute between former and new suppliers: the total number of French suppliers of the buyer rises after the recruitment, so that sales managers' transitions are not zero-sum. Second, the main loser from the new relationship between the buyer and firm 4 is supplier 1, the firm that lost a sales manager; suppliers 2 and 3 are virtually unaffected. Third, we propose an explanation for partial business-stealing: there is very little of this when the former and current employer of the sales manager sell different products. Overall, the transitions of sales managers across firms generate gains for French suppliers.

Figure 1.8 – Evaluating business-stealing



Notes: The left-hand side represents the buyer-seller relationships between French sellers and foreign buyers in 2005. The right-hand side represents the situation in time t . We observe in the data sales manager A moving from firm 1 to firm 4 in period t . Firms 1, 2 and 3 were selling to buyer B1 in 2005. The dashed blue arrows correspond to the relationship that we wish to evaluate: whether firms 1, 2 and 3 continue to sell to buyer B1 in period t .

1.5.1 Do the new buyer-supplier relationships come at the expense of other French suppliers of the buyer?

We evaluate the change in the number of French suppliers of a foreign buyer after the buyer establishes a new relationship with the firm that recruited the sales manager. In Figure 1.8, for example, buyer B1 starts importing from firm 4 after this firm hires sales manager A from firm 1. We then count the number of relationships buyer B1 has with French suppliers, after firm 4 starts selling to B1.

To carry out this analysis we collapse the data at the foreign buyer \times year level, with the outcome being the number of French suppliers of each foreign buyer b . The estimation equation for buyer b and year t is:

$$\text{No. French suppliers of buyer } b \text{ in year } t = \sum_{\substack{k=4 \\ k=-4 \\ k \neq -1}}^{k=4} \beta_k \mathbb{1}\{t = t_b + k\} + \gamma_b + \gamma_t + \epsilon_{fbt}$$

where t_b is the year in which buyer b forms a new relationship with firm 4 in Figure 1.8, *i.e.* with the French firm that recruited a sales manager from a firm selling to buyer b . γ_b and γ_t are respectively the buyer and year fixed effects.

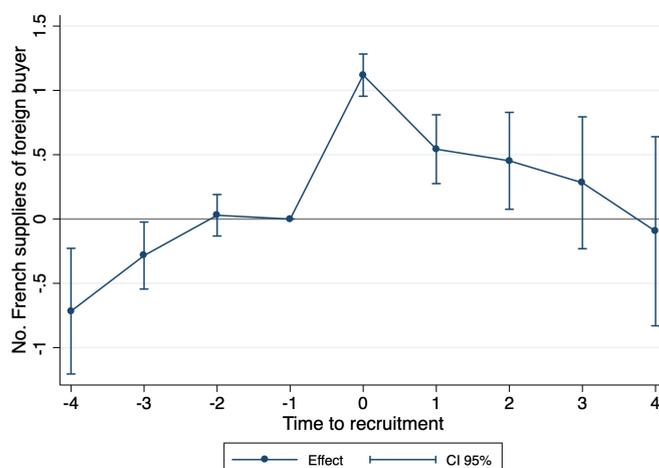
Figure 1.9 shows that in the recruitment year t , the buyer imports from 1.1 additional French suppliers, as compared to the year before the recruitment. This figure should be interpreted cautiously: in year t the buyer imports from a new supplier - firm 4 - by construction, but might have imported from its former suppliers previously that same year. Two years after the recruitment, the buyer imports from 0.45 additional French suppliers, as compared to $t - 1$. Thus, in the short-run, sales managers' job-to-job transitions do not lead to full business-stealing. In the longer-run, we no longer detect a significant effect of the recruitment on the buyer's number of French suppliers.

Figure 1.10 completes this picture and shows how the log value of the buyer's French imports changes after sales-manager job-to-job transitions: French imports rise by 11% one year after this transition.

One limitation of this exercise is that we do not have information on buyers' non-French suppliers. As a result, we can only assess whether sales managers' transitions are zero-sum at the *French level*, rather than at the *global level*.

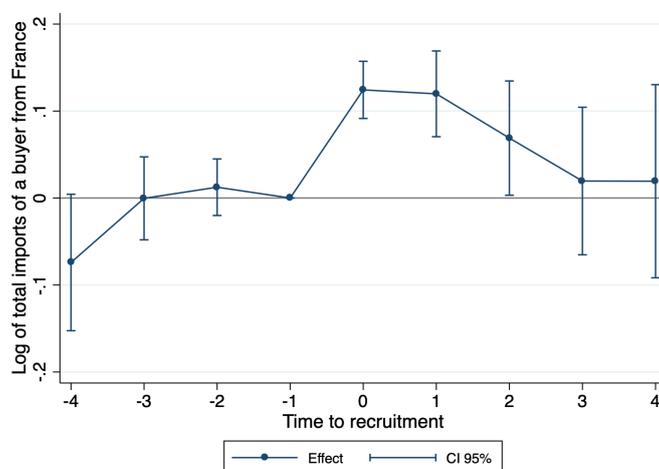
Note that the buyer's rise in the number of French suppliers, in Figure 1.9, is below one the year after the recruitment. As we condition on the buyer now importing from an additional supplier - firm 4 in Figure 1.8 - this new buyer-seller relationship necessarily comes at the expense of some of the buyer's former suppliers.

Figure 1.9 – Analysis at the buyer level: The effect of sales-manager recruitment on the buyer’s number of French suppliers - conditional on this buyer starting to import from the recruiting firm



Notes: The graph displays the effect of a new relationship between a foreign buyer and a French firm on the buyer’s number of French suppliers. We first select sales managers who move across French firms and who bring one of their former firm’s buyer to their recruiting firm. We analyze the number of French suppliers of the buyers before and after the sales manager’s job-to-job transition. One observation is one buyer \times year pair. Buyer and year fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the buyer level.

Figure 1.10 – The effect of sales-manager recruitment on a buyer’s total French imports - conditional on this buyer starting to import from the recruiting firm



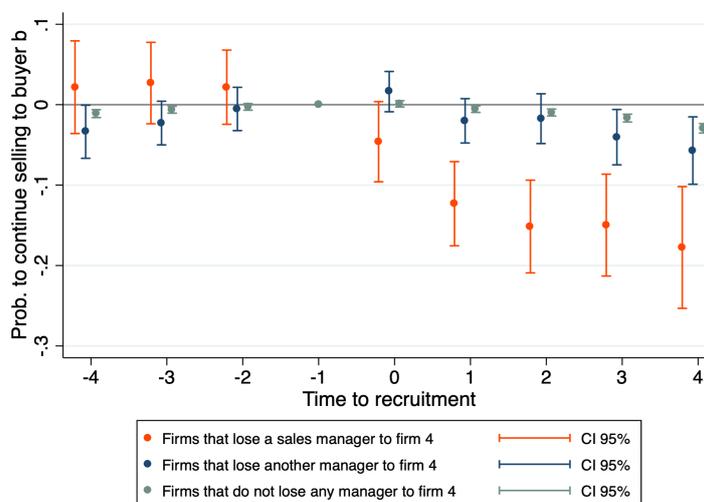
Notes: The graph displays the effect of a new relationship between a foreign buyer and a French firm on the buyer’s total French imports. We first select sales managers who move across French firms and who bring one of their former firm’s buyer to their recruiting firm. We analyze the total French imports of these buyers before and after the sales manager’s job-to-job transition. One observation is one buyer \times year pair. Buyer and year fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the buyer level.

1.5.2 Who loses from sales-managers’ job-to-job transitions?

The new buyer-seller relationship of a buyer can come at the expense of three different types of firms: those losing a sales manager to the buyer’s new supplier, those losing another type of manager to the buyer’s new supplier, and other former suppliers of the buyer. In the example of Figure 1.8, these are respectively firms 1, 2 and 3.

We analyze how the probability that each of these firms sell to their former buyer changes

Figure 1.11 – Business-stealing: The effect of a sales manager’s departure on the probability to sell to a buyer, conditional on this buyer starting to import from the recruiting firm



Notes: The graph displays the effect of a new relationship between a foreign buyer and a French firm on the buyer’s other suppliers. We select French firms selling to the buyer in 2005 and calculate the effect of the new buyer-seller relationship on the probability to sell to buyer b , k years before/after the departure. We estimate the effect separately for three types of firms: (i) firms losing a sales manager to the new supplier of the buyer (in orange), (ii) firms losing another type of manager to the new supplier (in blue) and (iii) other former suppliers of the buyer (in gray). The unit of analysis is the French firm \times foreign buyer pair. Firm \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level.

after the formation of the new buyer-seller relationship. We select French firms exporting to a given buyer at the beginning of the period - *i.e.* in 2005. The analysis is at the French firm \times foreign buyer \times year level, and the outcome variable is a dummy for the firm selling to the buyer. The estimation equation for firm f , buyer b and year t is:

$$\mathbb{1}(\text{firm } f \text{ sells to } b \mid \text{firm } f \text{ sells to } b \text{ in } 2005)_{fbt} = \sum_{\substack{k=-4 \\ k \neq -1}}^{k=4} \beta_k \mathbb{1}\{t = t_{fb} + k\} + \gamma_{fb} + \gamma_{ft} + \epsilon_{fbt}$$

where t_{fb} is the year that buyer b , which imported from French firm f in 2005, forms a new relationship with another French supplier. As in our Section 1.4 specification, we include firm \times year fixed effects γ_{ft} to control for unobserved shocks at the firm level that may shift both the probability of losing a sales manager and that of losing a foreign buyer. We also control for firm \times buyer fixed effects, γ_{fb} .

The results appear in Figure 1.11. When a sales manager leaves a firm and brings the buyer to her new firm, the probability that the former employer of the sales manager (firm 1 in Figure 1.8) sell to the buyer falls by 12.3 percentage points. This effect appears one year after the sales manager’s departure, and holds constant in the years thereafter. This effect is much larger than that in Section 1.4. By construction, we here restrict our analysis to French firm \times foreign buyer pairs that traded in the first year of our panel, in 2005. The effect here

should therefore be compared to the baseline probability of continuing to sell to a buyer in a given year, which is 80.7%.

The effect is much smaller for the other former suppliers of the buyers: firms 2 and 3 in Figure 1.8. As a placebo, we calculate the effect for suppliers that did not lose a sales manager but lost another type of manager to the buyer's new supplier in the year of the move: firm 2 in Figure 1.8. Here the probability of selling to the buyer falls by 2.0 percentage points one year after the sales manager's job-to-job transition. For the other suppliers of the buyer in 2005 (firms of type 3 in Figure 1.8), the probability of selling to the buyer is 0.56 percentage points lower one year after the sales-manager's job-to-job transition.

The new buyer-seller relationship thus mainly comes at the expense of the French supplier that lost a sales manager to the new supplier.

1.5.3 Why are sales-managers' job-to-job transitions not zero-sum?

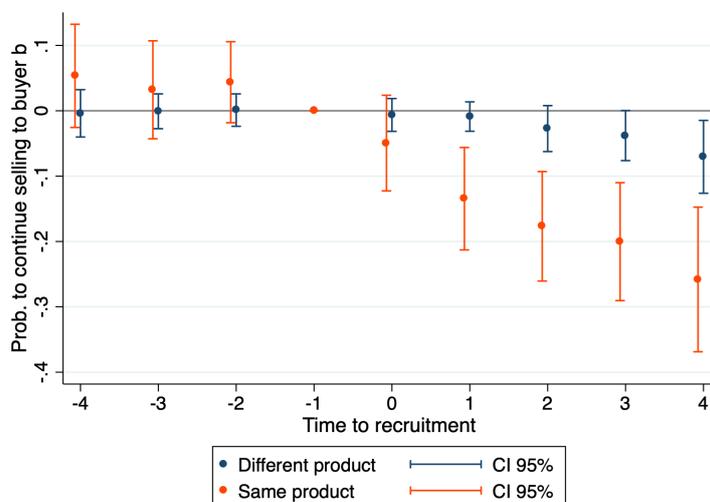
In the data, 90% of buyers purchase a given 8-digit product, in a given year, from a single French exporter. Business-stealing is therefore likely to be maximal when the sales-manager's current and previous employers sell the same 8-digit product. Our finding that job-to-job transitions are not zero-sum is thus a little puzzling. We here propose a potential explanation: sales-managers' transitions increase the span of products that the buyer imports from France.

First, conditional on the recruiting firm starting to sell to a buyer, this firm and the sales manager's former employer sell the same 8-digit product to this buyer in only 38% of cases. Second, when the products sold by these two firms to the buyer are not the same, business-stealing is almost non-existent. In Figure 1.12, losing a sales manager to a firm that starts selling product p to a buyer does not significantly affect the probability of selling a product p' to this buyer. By way of contrast, losing a sales manager to a firm selling product p to a buyer reduces the probability of selling product p to this buyer by 13.5 percentage points one year after the job-to-job transition.

These two results together indicate that sales-managers' transitions may create economic value when the initial and destination firms do not sell exactly the same products: sales managers' job-to-job transitions then increase the span of products buyers import from France. In this case, the effect of sales-managers' transitions goes beyond a reallocation across suppliers of a buyer, and increases the 'size of the pie'.¹⁸

¹⁸Note that even if sales managers' transitions merely reallocate suppliers across buyers, this can generate economic value if the new supplier is more productive than the previous supplier.

Figure 1.12 – Business-stealing: the effect of a sales manager’s departure on the probability to sell to a buyer, conditional on this buyer starting to import from the recruiting firm



Notes: The graph displays the effect of a sales manager’s departure on the probability of selling to buyer b , k years before/after the departure. We select pairs of French firms \times buyers such that: i) the French firm sells product p' to the buyer in 2005 and ii) the French firm suffers a worker’s departure toward the recruiting firm that starts to sell product p to the buyer in the year of the recruitment. The unit of analysis is the French firm \times foreign buyer \times product triple. Firm \times year and firm \times buyer fixed effects are included. We estimate the effect separately for observations where the product p' sold by the initial firm to the buyer in 2005 is the same as that sold by the recruiting firm to the buyer, and where the products are different. 95% confidence intervals are displayed. Standard errors are clustered at the firm level.

1.6 Mechanisms: What do sales managers know?

Why are sales managers able to bring some of their former firm’s clients with them when moving across firms? We first disentangle country- or product-knowledge from buyer-specific knowledge. We show in Section 1.6.1 that country- and product-knowledge explains under 20% of the business-network transmission effect, confirming that sales managers’ buyer-specific knowledge is central. We then dig deeper into the type of buyer-specific information that sales managers transmit: Section 1.6.2 presents suggestive evidence that personal relationships play a role in the creation of buyer-seller ties.

1.6.1 Is sales managers’ knowledge really buyer-specific?

Which type of knowledge matters for the creation of buyer-seller ties? We show that the dominant mechanism through which sales managers bring buyers with them when changing firms is that they hold buyer-specific knowledge accumulated in the previous job.

Sales managers may bring some of their former clients with them when changing firms, not via their buyer-specific knowledge, but rather country-specific or product-specific knowledge. A sales manager having exported textile materials to Denmark may be good at finding any textile importer in Denmark, irrespective of whether they have previously interacted with

them. Our main results would then reflect the specific export experience of sales managers, rather than their particular relationships with buyers.

We therefore analyze the effect for firms recruiting a sales manager connected to buyer b on the number of their new buyers in the same country and 8-digit product category as buyer b , excluding buyer b .¹⁹ We denote by c each country \times 8-digit product cell, and estimate the following equation:

$$\text{No. new buyers in } c \mid \text{recruiting from firm selling to cell } c_{fct} = \sum_{\substack{k=-4 \\ k \neq -1}}^{k=4} \delta_k \mathbb{1}\{t = t_{fc} + k\} + \gamma_{fc} + \gamma_{ft} + \gamma_{ct} + \epsilon_{fct}$$

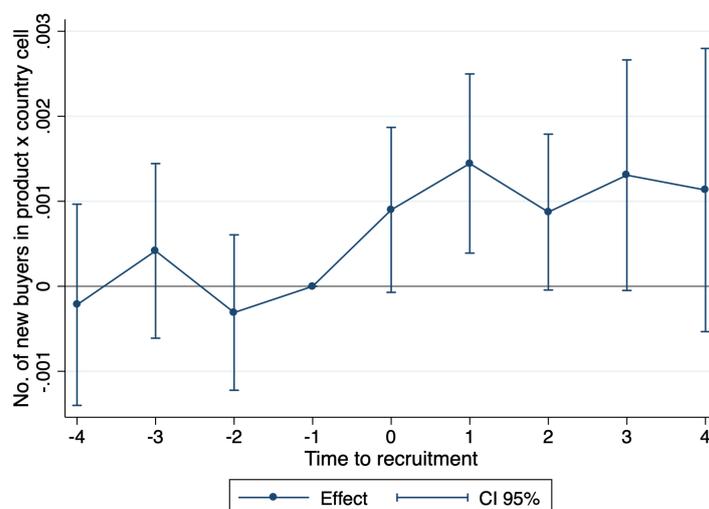
where t_{fc} is the year of recruitment by firm f of a sales manager working in a firm exporting to cell c (a firm exporting the same 8-digit product as b to the same country). The effect δ_0 divided by the number N_c of buyers in cell c yields the increase in the probability of selling to a given buyer b' in cell c as a result of the recruitment. If the knowledge required by a sales manager is not buyer-specific but entirely country \times product specific, we should find that $\frac{\delta_0}{N_c} = \beta_0$, where β_0 is our main effect estimated from Equation 1.1.²⁰

The results appear in Figure 1.13. There is a significant effect of the recruitment of a sales manager with expertise in the country \times product cell on the number of new buyers of the firm in this cell: at the year of the recruitment, the recruiting firm increases its number of new buyers in this cell by 0.23 percentage points. Crucially, this is not driven by the buyers connected to the sales managers, as these are excluded from the count of the number of buyers in this cell. This result confirms that sales managers transmit country \times product knowledge when they change employer: sales managers bring additional customers to the recruiting firm through their general knowledge of the market.

However, we find $\frac{\delta_0}{N_c} = 0.043$: recruiting a sales manager connected to buyer b increases the probability to start selling to a buyer in the same product \times country cell as b by 0.043 percentage points, corresponding to 18.7% of the higher probability to start selling to buyer

¹⁹This definition is very similar to that used in Section 1.4. We there removed pairs where the buyer is not a potential buyer for the firm. To be conservative, we define a potential buyer for a given firm as a buyer importing at least one product exported by the French exporting firm. In the current section, the approach is slightly different, as we classify each buyer by the largest product in terms of import volume. As a result, a given buyer pertains to only one 8-digit product \times country cell.

²⁰The analysis in this sub-section is at a different level to that in our core analysis in Section 1.4. We are here not at the firm \times buyer \times year level but at the firm \times cell \times year level, where a cell comprises several buyers. We thus cannot control for buyer \times year fixed effects, and instead control for cell \times year fixed effects. The coefficients cannot thus be compared directly to those in Section 1.4, given that the dimensionality for the identification differs. To address this issue, we devise a complementary strategy in Appendix 1.8.8 to carry out this sub-section's analysis at the firm \times buyer \times year level. We find that 8-digit product \times country knowledge explains 9% of our main results, instead of the 19% figure found when aggregating at the cell level. The results in this sub-section thus represent the upper bound of the importance of country \times product knowledge.

Figure 1.13 – The effect of recruiting a sales manager connected to buyer b on the number of new buyers in the same product \times country category as b 

Notes: The graph displays the effect for a firm of recruiting a sales manager connected to buyer b on the firm's number of new buyers in the same country and 8-digit product category as b , excluding buyer b . The data here is at the French firm \times product-country cell \times year level. The average number of buyers in a product \times country cell is 5.5. Dividing the effect displayed by 5.5 yields the increase in the probability to start selling to each buyer in a cell. Recruiting a sales manager connected to buyer b increases the probability to start selling to a buyer in the same product \times country cell by 0.043 percentage points, which corresponds to 18.7% of the increase in the probability to start selling to buyer b .

b . As such, country \times product knowledge does matter, but explains at most 20% of our main result. The dominant mechanism through which sales managers bring buyers with them when changing firms therefore seems to be their buyer-specific knowledge from their previous job.

Other levels of knowledge We have emphasized that 8-digit product \times country knowledge matters in order for firms to find buyers. We now compare the importance of this knowledge to that of broader types of knowledge: the results for other cell definitions appear in Table 1.4. Columns (1) to (4) refer to the estimation results for cells at the 8-digit product \times country, 6-digit product \times country, 4-digit product \times country, and 2-digit product \times country levels respectively. The more aggregated is the definition of a product, the smaller the effect of recruitment on the probability of starting to sell to a buyer in this cell. This is consistent with the recruitment of a sales manager connected to buyer b having a larger effect on the probability to start selling to buyer b' the closer are b and b' in terms of the products imported. For instance, in column (2), recruiting a sales manager connected to buyer b increases the probability to start selling to a buyer in the same 6-digit product \times country cell as b by 0.031 percentage points, corresponding to 13.6% of the increase in the probability to start selling to buyer b . This number falls to 2.9% for cells defined at the 2-digit \times country level. In column (5), we explore the pure product dimension: for cells at only the product level, we cannot detect any significant effect of recruitment on the new sales probability. The

same conclusion pertains for the pure country dimension in column (6).

Table 1.4 – The effect of recruiting a sales manager connected to buyer b on the number of new buyers in the same country \times product category as b

	(1) 8-digit product x country	(2) 6-digit product x country	(3) 4-digit product x country	(4) 2-digit product x country	(5) 8-digit product cell	(6) Country cell
t-4	-0.022 (0.060)	0.003 (0.058)	-0.115 (0.086)	-0.146 (0.173)	0.104 (0.174)	-0.937 (0.737)
t-3	0.042 (0.052)	0.062 (0.047)	-0.004 (0.072)	0.100 (0.163)	0.227 (0.113)	-0.451 (0.614)
t-2	-0.031 (0.047)	0.020 (0.050)	-0.053 (0.071)	-0.047 (0.151)	-0.012 (0.099)	0.360 (0.626)
t	0.090* (0.045)	0.066 (0.046)	0.068 (0.067)	0.095 (0.143)	-0.011 (0.093)	-0.297 (0.533)
t+1	0.144*** (0.054)	0.125** (0.052)	0.114 (0.743)	0.367* (0.214)	-0.059 (0.113)	0.490 (0.508)
t+2	0.087* (0.047)	0.117** (0.049)	0.095 (0.101)	0.168 (0.191)	0.006 (0.131)	0.445 (0.595)
t+3	0.131* (0.069)	0.139** (0.066)	0.189 (0.098)	0.289 (0.192)	-0.086 (0.167)	0.901 (0.705)
t+4	0.113 (0.085)	0.125 (0.085)	0.196* (0.118)	0.300 (0.235)	-0.124 (0.242)	1.884** (0.790)
No. obs	3,734,772	3,717,540	3,286,392	2,196,012	2,449,584	442,452
Avg. no. of buyers in cell	5.45	6.13	12.46	70.63	28.10	5,224.71
\nearrow in prob. to start selling to buyer in same cell as b - in p.p.	0.043	0.031	0.015	0.007	n.a.	3.7×10^{-5}
% of main effect	18.7%	13.6%	6.4%	2.9%	n.a.	n.a.

Notes: This table shows the effect for a firm of recruiting a sales manager connected to buyer b on the firm's number of new buyers in the same cell as b , excluding buyer b . We carry out this estimation for different cell definitions: columns (1) to (4) respectively use the definition of products at the 8-digit level, 6-digit, 4-digit and 2-digit levels, interacted with country. Column (5) uses only information on the 8-digit product of buyers, without using information on the country. Column (6) uses only information on the country of buyers, without using information on the product. The effects are shown in percentage points, with standard errors in parentheses. We calculate the increase in the probability to start selling to a buyer in the same cell as b as follows: we calculate the cumulative effect in t and $t + 1$ of the recruitment, and divide this sum by the average number of buyers in a cell. We then compare this higher probability of starting to sell to a buyer in the same cell as b to the increase in probability to start selling to buyer b , which corresponds to our main event-study figure estimated in Equation 1.1. In column (2), the table reads as follows: recruiting a sales manager connected to buyer b increases the probability to start selling to a given buyer in the same 6-digit product category and country as b by 0.031 percentage points. This corresponds to 13.6% of the increase in the probability to start selling to buyer b . As such, 6-digit product \times country knowledge explains approximately 13.6% of our main event-study figure.

1.6.2 Personal relationships between the sales manager and buyer

We now investigate the nature of the sales managers' buyer-specific knowledge. We provide suggestive evidence that personal relationships between sales managers and buyers matter.

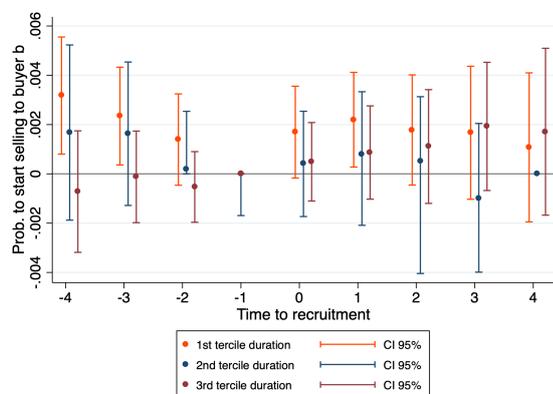
Contracting frictions are important impediments to trade (see Antràs and Helpman (2006) and Startz (2016)), and trust has been suggested as one way of overcoming these frictions (see Casella and Rauch (2002)). It is, however, difficult to distinguish between information and trust empirically. The duration of the relationship between the buyer and the poached firm provides us with some idea about the number of exchanges between the sales manager and the buyer, and thus the strength of their personal relationship. However, this is likely to be

correlated with the relationship-specific investments between the buyer and the poached firm, which are themselves negatively correlated with the propensity of a buyer to switch suppliers.

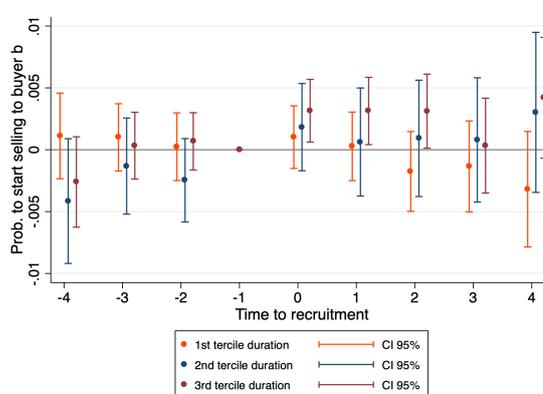
One proxy for the experience of the sales manager with the buyer is the sales manager's age interacted with the duration of the relationship between the buyer and the poached firm. We confirm that the older the sales manager, the greater the recruitment effect, see Appendix Figure 1.8.16a.²¹

Figure 1.14 shows that the recruitment of younger sales managers has a positive significant effect on the probability to sell to a buyer only when the duration of the relationship between the buyer and the poached firm is sufficiently short. The opposite pattern holds for older sales managers: the recruitment of a sales manager is only effective for long-duration relationships. This cannot be explained by a pure matching-friction story: were sales managers to only bring information on the buyer's identity to their new firm, we would see no difference for older and younger sales managers. This suggests that the development of personal relationships between buyers and sales managers may play a role in firm-buyer ties.

Figure 1.14 – The effect of recruiting a sales manager connected to a buyer - older vs. younger sales managers



(a) Younger sales managers



(b) Older sales managers

Notes: The graph displays the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b k years before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. We display the effects for younger and older sales managers (with the threshold being age 35), according to the duration of the relationship between the poached firm and the buyer prior to sales-manager departure. Tertiles of durations correspond to relationships lasting under 1.25 years, 1.25 to 4 years, and 4 years or more.

²¹This holds even when controlling for the hierarchy of the sales manager's occupation, thus confirming that this age effect is not driven by occupation.

1.7 Conclusion

This paper is the first to explore the role of sales managers in firms' international expansion via their construction of a firm's buyer portfolio. Our first contribution is to examine the channel through which sales managers help firms meet new buyers. We provide novel causal evidence that sales managers bring some of their former firm's clients to their new firm when recruited. Our second contribution is to explore the implications of sales managers' job-to-job transitions on the sales manager's previous employer: we find that these transitions are not zero-sum, suggesting that sales managers create economic value for French suppliers.

Sales-managers' job-to-job transitions are not exogenous in our setting, and may thus be related to worker or firm characteristics. On average in our data, sales managers moving across French exporting firms tend to go to more productive, higher-paying and younger firms. The fact that this reallocation of sales managers toward better firms generates economic gains raises interesting issues related to the use of non-compete agreements. On the one hand, these contracts that limit workers' mobility across firms preclude the reallocation of workers to better firms and restrict firm entry. On the other hand, they have the merit of increasing firms' investments in their workers' human capital. Our results shed light on the anti-competitive effects of non-compete agreements by focusing on one aspect of firm performance: the firm's number of foreign buyers.

Our paper also informs about the type of search frictions that selling firms face on international goods markets. Our results imply that match-making services are of first-order importance for firms to find buyers. In particular, priority should be placed on services that favor frequent in-person meetings between the managers of buying and selling firms, thereby potentially establishing personal relationships. However, our work also indicates that such policies might come at the expense of the incumbent firms: helping small productive firms find new buyers will likely be to the detriment of the foreign buyers' current suppliers.

1.8 Appendices

1.8.1 Appendix A: Customer accumulation is a key driver of firm-level export growth

We investigate the drivers of the heterogeneity in export growth across exporters. We decompose the variance of firm-level export growth - over various horizons²² - into four net margins, as illustrated in Equation (1.2). First, the net product margin encompasses sales on added products with continuing buyers minus sales on dropped products for buyers still in the sellers' portfolio. The second is the buyer margin, which represents the evolution of exports related to the change in the customer base - added and dropped buyers - within a stable portfolio of products. The third margin concerns the simultaneous addition or dropping of a buyer-product couple. Namely, when a firm simultaneously adds or drops a product and a buyer to its portfolio, we add its sale to this margin. The last margin is the evolution of sales for buyers and products that are present in the firm's portfolio over the two periods.

$$\Delta Exports_{ft} = \Delta Products + \Delta Customer Base_{ft} + \Delta Product \times Buyer + \Delta Intensive Margin \quad (1.2)$$

One concern is that $\Delta Exports_{ft}$ is highly correlated with firms size. In the decomposition above, large firms will therefore be attributed a high weight. To alleviate this concern, we choose to decompose the export growth rate as follows:

$$\begin{aligned} \frac{2 \times \Delta Exports_{ft}}{(Exports_{ft-1} + Exports_{ft})} &= \frac{2 \times \Delta Products}{(Exports_{ft-1} + Exports_{ft})} + \frac{2 \times \Delta Customer Base_{ft}}{(Exports_{ft-1} + Exports_{ft})} \\ &+ \frac{2 \times \Delta Product \times Buyer}{(Exports_{ft-1} + Exports_{ft})} + \frac{2 \times \Delta Intensive Margin}{(Exports_{ft-1} + Exports_{ft})} \end{aligned}$$

We then regress without a constant each of these margins on the growth of exports controlling for year fixed effects in order to pick up specific time shocks.²³

$$\Delta Margin = \beta \Delta Exports_{fct} + \delta_{ct} + \epsilon_{fct} \quad (1.3)$$

We underline in this section a novel empirical finding about export growth conditional on survival: while the intensive margin matters in the short-run, the buyer margin is the main

²²We here abstract from the country margin as this comes down to adding a new buyer in a new destination.

²³In a decomposition without fixed effects, the β coefficient is exactly the share of export-growth variance across firms that is explained by the margin considered.

contributor to export growth in the medium and long-run.

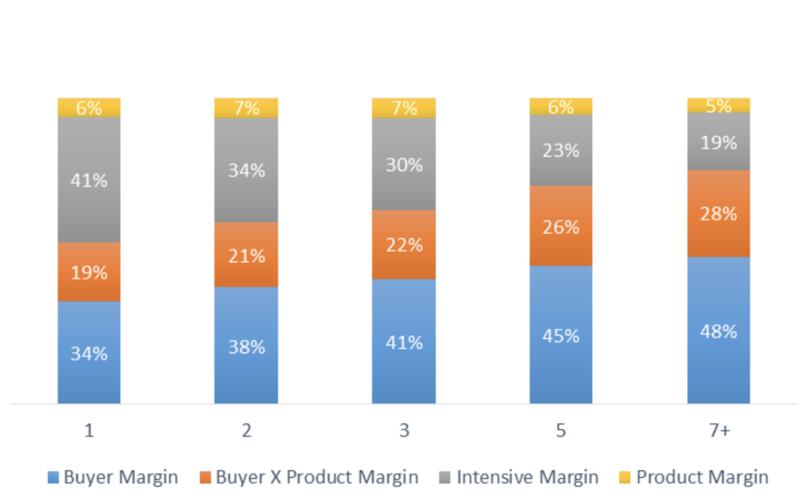
Figure 1.8.1 displays the decomposition of export growth conditional on survival into the different margins described above;²⁴ we show for each margin the β coefficient in Equation 1.3. We carry out this decomposition for different time horizons. The decompositions of 1-year and 7-year export growth reveal starkly opposing patterns: the buyer margin explains 34% of 1-year export-growth heterogeneity and 48% of 7-year export growth variance. When including the buyer \times product margin these figures range from 53% for 1-year growth to 76% for 7-year growth. The intensive margin contributes to the bulk of the short-term growth variance, as it explains 41% of 1-year growth variance, but it is of secondary importance in the medium- and long-run: the intensive margin explains 30% of 3-year growth variance, which figure falls to 19% for 7-year growth variance. The different contributions of the margins to short- and long-term export growth are not driven by composition effects related to the duration of firms into international markets. The declining importance of the intensive margin and the rising relevance of the buyer margin are still found when we restrict our variance-decomposition analysis to exporters that were present in the export market for at least 5 years.

The pure product margin contributes very little to the explanation of export-growth heterogeneity. This indicates that firms do not grow by selling new products to their current buyers, but rather either extend their portfolio of buyers or sell new products to new buyers. In the medium and long run, this second mechanism seems to be important in explaining export-growth heterogeneity, as its explanatory power is higher than that of the intensive margin.

To check that the effects above are not driven by sales to new countries, we further decompose the buyer margin into the country margin - sales to new countries for a given firm - and the pure buyer margin - sales to new buyers within existing countries. Figure 1.8.2 displays the results. At the 1-year horizon, the total buyer margin is accounted for at 44% by the country margin, computed as the ratio of the contribution of the buyer margin - 19% - divided by the total buyer margin - 15%+19%. This figure drops to 40% at the 7-year horizon. Even without including the country margin, the buyer margin is still dominant in explaining export growth: this margin explains 19% of 1-year growth variance, rising to 29% for the 7-year growth variance.

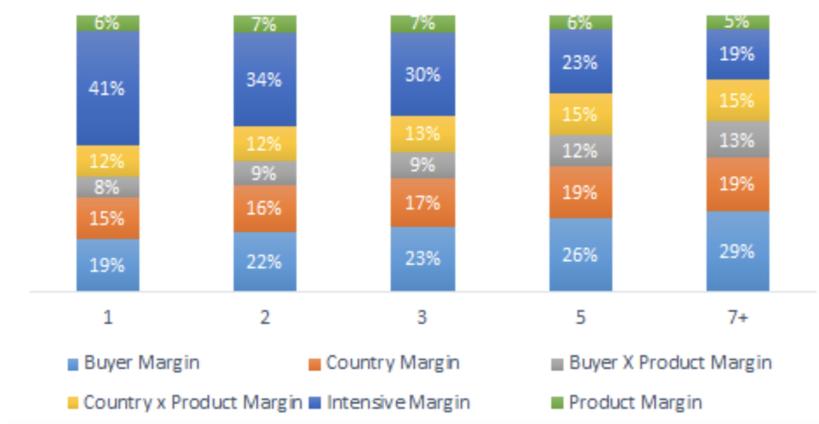
²⁴Products are defined at the 8-digit level in an harmonized nomenclature, and growth rates above the 99th percentile are replaced as missing.

Figure 1.8.1 – The variance decomposition of export growth



Notes: This graph displays the decomposition of the variance of export growth into four margins: i) the buyer margin, ii) the buyer × product margin, iii) the intensive margin, and iv) the product. To do so, we regress each firm-level margin growth on total export growth, as shown in Equation 1.3. Buyer-margin growth in year t in a given firm is constituted by the sales of this firm to new buyers, minus the sales of this firm in $t - 1$ to buyers it does not serve anymore. The graph reads as follows: at the 1-year horizon, the buyer margin explains 34% of the heterogeneity in export growth across exporters.

Figure 1.8.2 – The variance decomposition of export growth - decomposing the buyer margin into the country margin and the pure buyer margin



Notes: This graph displays the decomposition of the variance of exports' growth into six margins: i) the buyer margin, ii) the country margin, iii) the buyer × product margin, iv) the country × product margin, v) the intensive margin, and vi) the product margin. To do so, we regress firm-level margin growth on total export growth, as in Equation 1.3. Buyer-margin growth in year t for a given firm is the sales of this firm to new buyers, minus the sales of this firm in $t - 1$ to buyers it does not serve anymore. The graph reads as follows: at the 1-year horizon, the buyer margin explains 19% of the heterogeneity in export growth across exporters.

1.8.2 Appendix B: Descriptive statistics on sales managers

Which sectors have a larger share of sales managers?

Tables 1.8.1 and 1.8.2 list the 10 sectors, at the 2-digit level, with the highest and lowest share of sales managers in total employment. Unsurprisingly, retail and wholesale trade are sectors with the most sales managers in their employment. However, every 2-digit sector employs a non-zero share: sales managers are not restricted to specific sectors, but are rather ubiquitous.

Table 1.8.1 – The 10 sectors with the highest share of sales managers in employment

2-digit sector label	Share of sales managers in total employment
Advertising and market research	26.7%
Wholesale trade, except for motor vehicles and motorcycles	26.1%
Motion picture, video and television programme production	23.3%
Wholesale and retail trade of motor vehicles	18.2%
Retail trade, except for motor vehicles	16.1%
Rental and leasing activities	16.1%
Activities of head offices, management consultancy activities	14.1%
Manufacture of beverages	13.6%
Publishing activities	12.6%
Office administrative and other business support activities	12.2%

Notes: This table lists the 10 French 2-digit sectors with the highest percentage of sales managers in total employment. The statistics are for exporting firms in 2012.

Table 1.8.2 – The 10 sectors with the lowest share of sales managers in employment

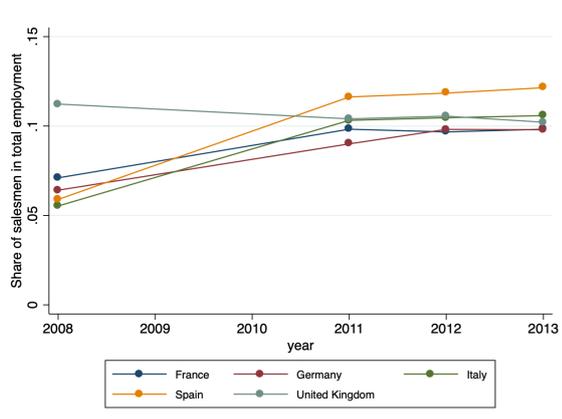
2-digit sector label	Share of sales managers in total employment
Land transport and transport via pipelines	0.2%
Manufacture of motor vehicles, trailers and semi-trailers	0.3%
Manufacture of basic metals	0.3%
Waste collection and materials recovery	0.4%
Manufacture of other transport equipment	0.4%
Manufacture of fabricated metal products	0.4%
Manufacture of pulp, paper and paperboard	0.5%
Manufacture of wood and of product of wood and cork	0.5%
Other mining and quarrying	0.5%
Manufacture of leather and related products	0.5%

Notes: This table lists the 10 French 2-digit sectors with the lowest percentage of sales managers in total employment. The statistics are for exporting firms in 2012.

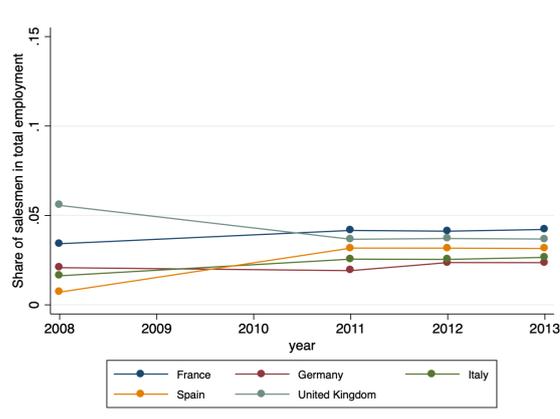
Comparison to other datasets

We here define sales managers using the French classification of occupations. We compare the numbers using our definition to those from the international classification of occupations. We can define sales managers in the ISCO 3-digit classification as workers with the following ISCO occupational codes: ‘122’ = ‘Sales, Marketing and Development Managers’, ‘243’ = ‘Sales, Marketing and Public Relations Professionals’ and ‘332’ = ‘Sales and Purchasing Agents and Brokers’. The occupation codes ‘521’, ‘522’, ‘523’ and ‘524’ correspond respectively to ‘Street and Market Salespersons’, ‘Shop Salespersons’, ‘Cashiers and Ticket Clerks’ and ‘Other Sales Workers’. These are sales workers occupations that we exclude from our analysis, as they correspond to lower-skilled positions that are not likely to have an impact on a firm’s foreign buyers. We use data on European Labour Force Surveys to calculate the broad share of sales workers - as the sum of codes 122, 243, 332, 521, 522, 523 and 524 - in total employment in a number of European countries. We also calculate a restricted share of sales managers, dropping low-skilled sales workers, covering only codes 122, 243 and 332. The results appear in Figure 1.8.3 below. Sales workers correspond to 9.8% of French employment in 2013, and sales managers to 4.2%. These numbers are consistent with what we find in the *DADS Postes* data. The share of sales managers is relatively similar across countries, and stable over time.

Figure 1.8.3 – Share of sales workers and sales managers in total employment - European Labour Force Survey data



(a) Sales workers and sales managers



(b) Sales managers only

Notes: EU-LFS 2008-2013.

1.8.3 Appendix C: Additional stylized facts

Estimation results

Table 1.8.3 shows the results from the regressions behind Figure 1.2a. We regress the firm's log number of buyers on the log number of sales managers. Columns (1) to (5) include firm, sector \times year and employment zone \times year fixed effects. Column (1) includes no other controls. Column (2) then introduces firm log employment: unsurprisingly, this substantially reduces the coefficient on the log number of sales managers. Columns (3), (4) and (5) then add productivity controls and the firm's log number of managers. The results are not particularly sensitive to the inclusion of these controls.

Table 1.8.3 – Regression of the log No. of buyers on the log No. of sales managers

	(1)	(2)	(3)	(4)	(5)
	log(# buyers)	log(# buyers)	log(# buyers)	log(# buyers)	log(# buyers)
log # sales managers	0.110*** (0.00404)	0.0268*** (0.00409)	0.0291*** (0.00407)	0.0319*** (0.00451)	0.0269*** (0.00446)
log employment		0.287*** (0.00650)	0.131*** (0.00771)		0.127*** (0.00790)
log value added			0.227*** (0.00719)		0.227*** (0.00719)
log value added/worker			-0.131*** (0.00673)		-0.132*** (0.00673)
log # qualified workers				0.155*** (0.00520)	0.00648 (0.00576)
# obs	311,434	311,434	299,853	311,434	299,853
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector \times year FE	Yes	Yes	Yes	Yes	Yes
CZ \times year FE	Yes	Yes	Yes	Yes	Yes

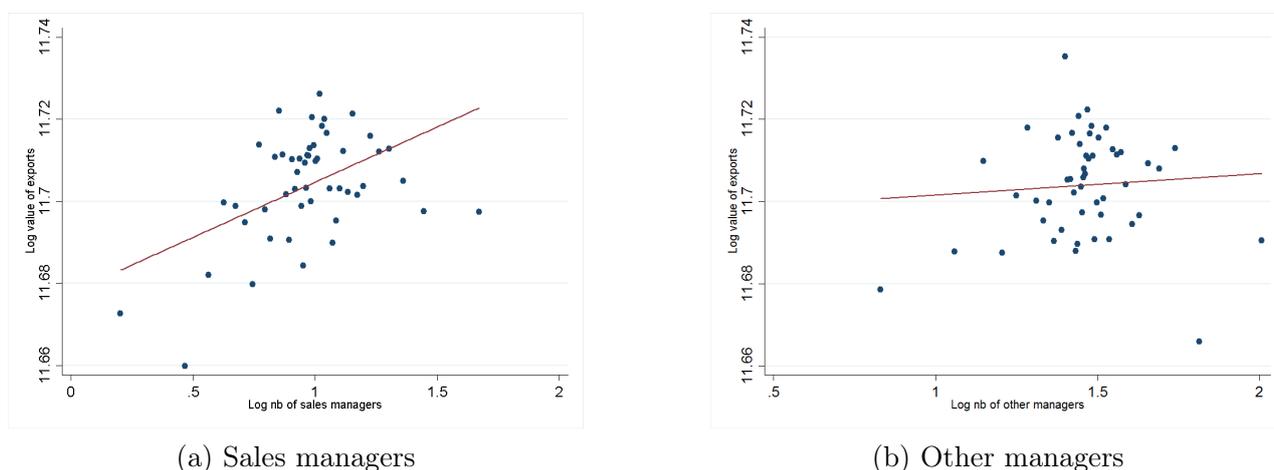
Notes: Data source: French matched employer-employee data merged with Customs data, 2009-2015. The dependent variable is the firm's log of the number of buyers. The independent variable of interest is the log number of sales managers in the firm plus one, to account for zeros. We control for firm log employment, log value added, log value added per worker and log of the number of managers. Column (5) produces the results in Figure 1.2a. Firm, sector \times year and commuting zone \times year fixed effects appear in all columns. [Go back to main text](#)

The margins of trade

We depict below the correlations between the firm's number of sales managers and its different margins of trade: i) total exports, ii) the number of products exported - product margin - and iii) the value exported for each product to each buyer - the intensive margin. We also show the same correlations for the number of other types of managers in the firm. The results appear in Figures 1.8.4, 1.8.5 and 1.8.6. The firm's number of sales managers is positively correlated with the firm's total exports and number of exported products, but negatively correlated with the intensive margin (the value of exports per buyer per product). The role of sales managers

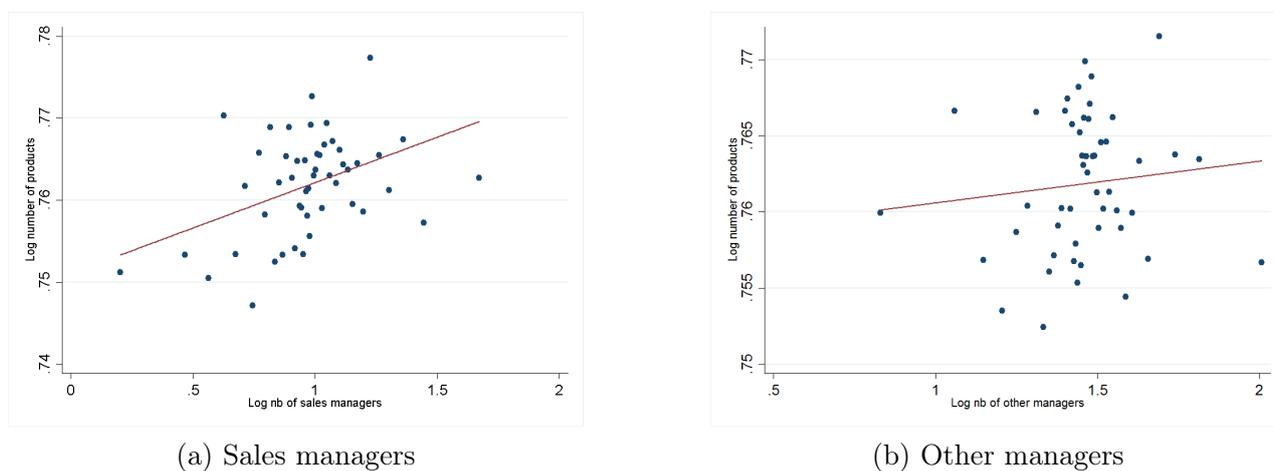
is then predominantly to increase the extensive margin of exports, by selling either to more buyers or selling more products. The size of the correlation between the number of sales managers and the number of products is lower than that with the number of buyers. There is no correlation between the number of other managers and any of the export margins.

Figure 1.8.4 – Total exports versus number of managers in the firm



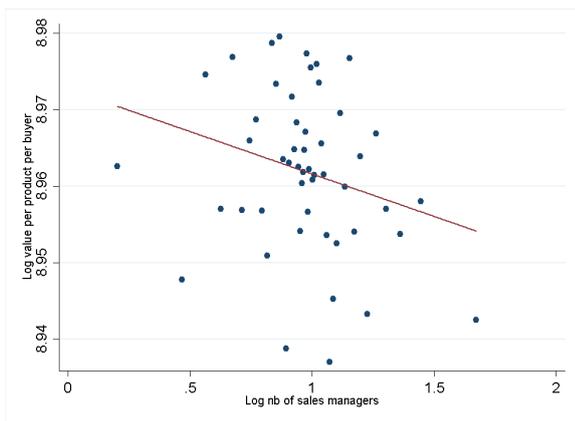
Notes: The figures come from Customs data merged with matched employer-employee data 2009-2015. Controls: firm lagged employment, lagged number of managers, lagged value added and value added per worker, and firm, sector \times year and employment zone \times year fixed effects. [Go back to main text](#)

Figure 1.8.5 – The number of products exported versus the number of managers in the firm

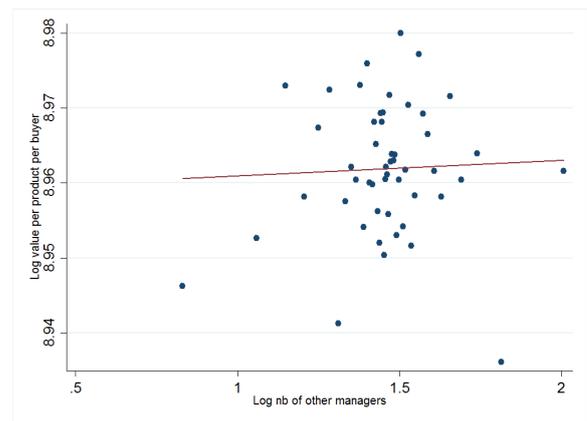


Notes: The figures come from Customs data merged with matched employer-employee data 2009-2015. Controls: firm lagged employment, lagged number of managers, lagged value added and value added per worker, and firm, sector \times year and employment zone \times year fixed effects. Products are measured at the 6-digit level. [Go back to main text](#)

Figure 1.8.6 – Export value for each product and buyer - *intensive margin of exports* versus the number of managers in the firm



(a) Sales managers



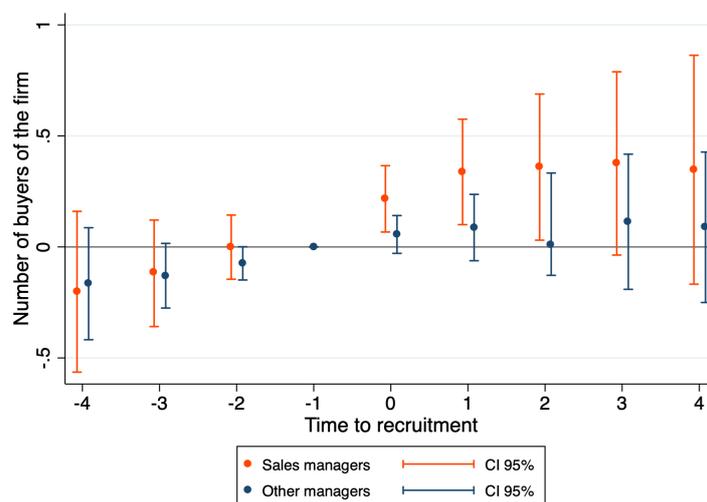
(b) Other managers

Notes: The figures come from Customs data merged with matched employer-employee data 2009-2015. Controls: firm lagged employment, lagged number of managers, lagged value added and value added per worker, and firm, sector \times year and employment zone \times year fixed effects. [Go back to main text](#)

Sales-manager recruitment and the number of foreign buyers

The positive correlation between the number of sales managers and the firm's number of buyers suggests that sales managers play a role for firms in selling to buyers, but it could also be that firms with more buyers hire more sales managers to cater to these buyers. To have a better sense of the timing of events, we look at the change in the firm's number of buyers before and after sales-manager recruitment. We carry out an event-study analysis for firms recruiting sales managers at most once over the 2009-2015 period. We use a staggered adoption design, in that only treated firms are included in the sample, and the control firms are those that have not yet recruited. Figure 1.8.7 shows the results separately for sales managers and other types of managers. Sales-manager recruitment is associated with an 0.22 rise in the firm's number of buyers, corresponding to a 7.6% increase over the baseline figure. This association is insignificant and of much smaller size for other types of managers. When restricting the analysis to exporters, the figure is 0.85, corresponding to a 4.0% rise over the baseline figure in this sample. These event-study results should not however be over-interpreted, as they are not causal: firm-level shocks, such as demand shocks, may be correlated with both the firm's number of buyers and its recruitment behavior.

Figure 1.8.7 – The recruitment of managers and the number of foreign European buyers



Notes: The figure shows the results of event-study analysis for firms recruiting sales managers exactly once over the 2009-2015 period. We use a staggered adoption design, in that only treated firms are included in the sample, and the control firms are those that have not yet recruited. Firm fixed effects are included. Standard errors are clustered at the firm level. [Go back to main text](#)

1.8.4 Appendix D: The De Chaisemartin-D'Haultfoeuille (2020) estimator

The de Chaisemartin and d'Haultfoeuille (2020) estimator works as follows. The coefficient at the date of treatment t is estimated by comparing the evolution of the outcome between $t - 1$ and t for units that are treated in t to that for units that are treated in $t + 1$ or later. Similarly, the coefficient at $t + 1$ is obtained by comparing the evolution of the outcome between $t - 1$ and $t + 1$, for units that are treated in t and those that are treated in $t + 2$ or later.

In our setting, treatment (the recruitment of a sales manager by firm f connected to buyer b) may occur in any year over the [2009,2015] period. Formally, the estimator for the effect of recruiting four years after the treatment is a weighted average of the following Difference-in-Differences:

$$DiD_1 = (Y_{2013} - Y_{2008}|t_{fb} = 2009) - (Y_{2013} - Y_{2008}|t_{fb} \geq 2014)$$

$$DiD_2 = (Y_{2014} - Y_{2009}|t_{fb} = 2010) - (Y_{2014} - Y_{2009}|t_{fb} \geq 2015)$$

where t_{fb} is the year of the recruitment by firm f of a sales manager connected to buyer b and Y_{2009} is the outcome variable in 2009. The first Difference-in-Difference (DiD_1) measures the evolution of the outcome variable between 2008 and 2013, for the units treated in 2009 and the units treated in 2014 or later. In the second DiD, the control group comprises units treated in 2015. The effect of the treatment is estimated with fewer and fewer observations the more years after the recruitment, thus generating higher standard errors further away from the recruitment.

The authors also devise a placebo estimator to test for pre-trends. For instance, the placebo estimate in year $t - 1$ compares the outcome's evolution from $t - 2$ to $t - 1$, in groups that switch and do not switch treatment between $t - 1$ and t . Formally, the placebo estimator in $t - 1$ is the weighted average of the six following Difference-in-Differences:

$$DiD_1 = (Y_{2008} - Y_{2007}|t_{fb} = 2009) - (Y_{2008} - Y_{2007}|t_{fb} \geq 2010)$$

$$DiD_2 = (Y_{2009} - Y_{2008}|t_{fb} = 2010) - (Y_{2009} - Y_{2008}|t_{fb} \geq 2011)$$

$$DiD_3 = (Y_{2010} - Y_{2009}|t_{fb} = 2011) - (Y_{2010} - Y_{2009}|t_{fb} \geq 2012)$$

$$DiD_4 = (Y_{2011} - Y_{2010}|t_{fb} = 2012) - (Y_{2011} - Y_{2010}|t_{fb} \geq 2013)$$

$$DiD_5 = (Y_{2012} - Y_{2011}|t_{fb} = 2013) - (Y_{2012} - Y_{2011}|t_{fb} \geq 2014)$$

$$DiD_6 = (Y_{2013} - Y_{2012}|t_{fb} = 2014) - (Y_{2013} - Y_{2012}|t_{fb} \geq 2015)$$

Note that we have added fixed effects to the de Chaisemartin and d'Haultfoeuille (2020) method: we remove buyer and firm-level trends thanks to firm \times year fixed effects and buyer \times year fixed effects. We refer the reader to the online Appendix of de Chaisemartin and d'Haultfoeuille (2020) for more details about the inclusion of fixed effects in the authors' estimator.

1.8.5 Appendix E: The construction of the sample of sales-manager recruitments

Potential buyers

We only retain the potential buyers for the French firm under consideration. We consider as potential products the 8-digit level products - harmonized as described in Bergounhon et al. (2018) - that the French firm exports during the 2005-2017 period. We drop products that do not represent, within a sector category, at least 10% of the exports of one French firm. We use a similar process to define a buyer's potential products. The potential buyers of a French firm are then defined as all buyers importing at least once one of the French firm's potential products. Despite the 68% drop in buyer-supplier pairs, this restriction is fairly conservative, as only 0.23% of the removed buyer-seller pairs match at some point in the period.²⁵

Excluding firm deaths, mergers and absorptions

We exclude cases in the data that likely correspond to firm death, mergers or absorptions.

The poached firm is dying We exclude situations in which the poached firm is potentially dying. A large negative shock to a firm simultaneously spurs the movement of workers to its competitors and of its buyers to its competitors. In this case, the effect of a sales-manager hire on the probability to sell to a former buyer of the sales manager very likely stems from the correlated effects between sales-manager departure and buyer departure. We exclude these problematic cases by dropping sales-manager movements for which the poached firm exhibits a fall of over 60% of its workforce the year of the sales-manager recruitment.

The recruiting firm absorbs the poached firm We remove from the sample worker flows that have the three following characteristics: i) the worker flow from firm A to firm B is over 70% of firm A's employment before the move, ii) firm A has more than five workers the year before the move, and iii) firm A has fewer than two employees the year of the move. These cases correspond to situations in which the administrative identifier of firm A has probably been changed into firm B. This may correspond to an absorption of firm A by firm B, a merger, or a simple administrative change of identifier. In this case we take the conservative approach of excluding both firm A and from B from the sample.

²⁵This figure is 4.60% for buyer-seller pairs that are defined as potential.

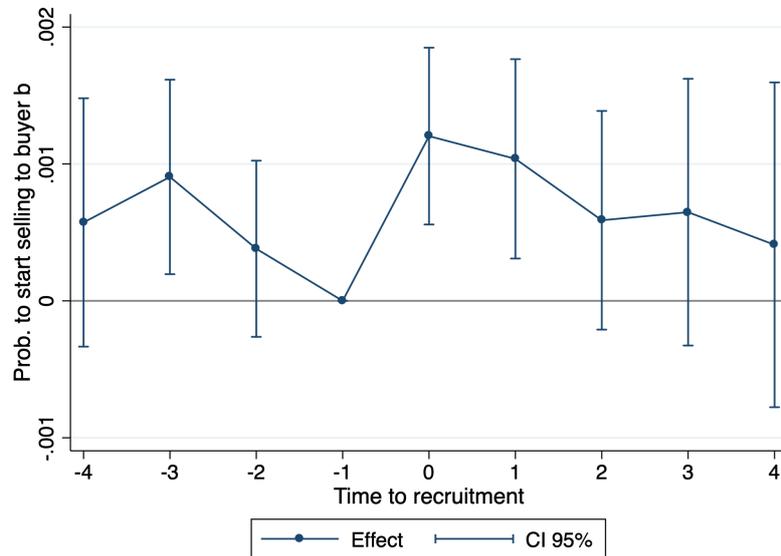
The recruiting firm absorbs an establishment of the poached firm We apply the exact same procedure as above at the establishment level: if the flow from establishment A to establishment B corresponds to over 70% of establishment A's employment before the move, if establishment A has over five workers the year before the move, and if establishment A has no employment in t (meaning that the identifier may have changed), then we consider that the identifier of establishment A has been changed into establishment B. Then, if the year of the sales-manager movement from firm 1 to 2, at least one establishment of firm 1 is bought by firm 2, we remove this worker move from the sample, as it may correspond to a potential merger between any two establishments in these firms.

The sample of buyers

The way in which we constitute our sample may produce some selection bias. We select firm $f \times$ buyer pairs such that, in year t , a sales manager moves from an initial firm f' to f . A buyer is thus included in our sample if it buys from at least one French supplier from which at least one sales manager leaves. Buyers with positive unobserved shocks are thus more likely to appear in our sample. However, even conditional on being selected, the year of the sales-manager movement likely corresponds to the largest positive unobserved shock faced by the buyer in the period. As a result, year $t - 1$, where t designates the year of the sales-manager movement, corresponds to a peak of imports for the buyer, which may create issues in an event-study setting in which the outcome in all other years is compared to that at $t - 1$. A simple way of solving this issue is to control for buyer \times year fixed effects, which remove unobserved buyer-level time-variant shocks.

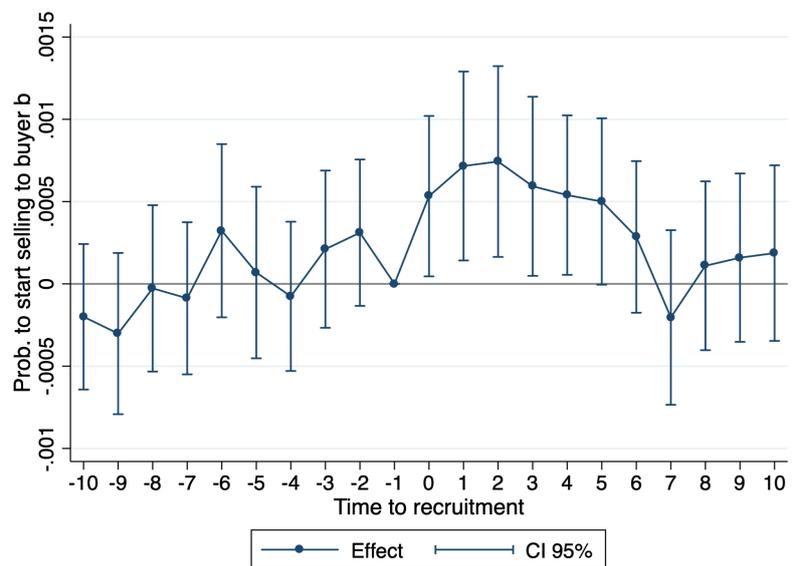
1.8.6 Appendix F: Robustness tests

Figure 1.8.8 – The effect of recruiting on the probability to sell to a buyer - controlling for firm-market trends



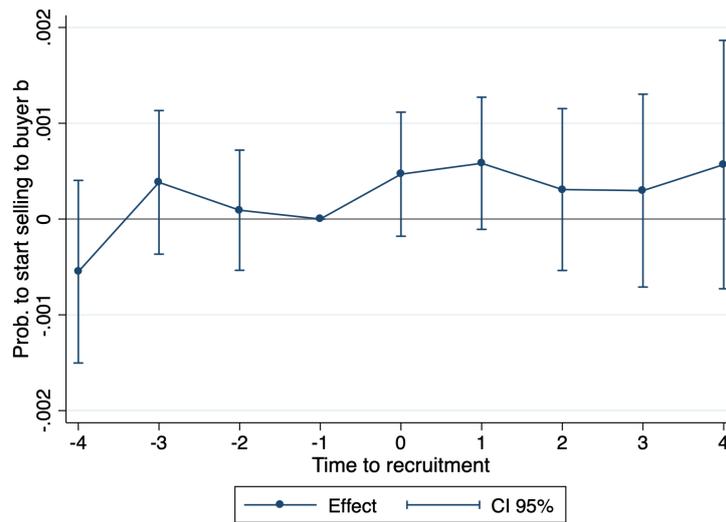
Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Equation (1.1), which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b k quarters before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times country \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

Figure 1.8.9 – The effect of recruiting on the probability to sell to a buyer - quarterly estimation

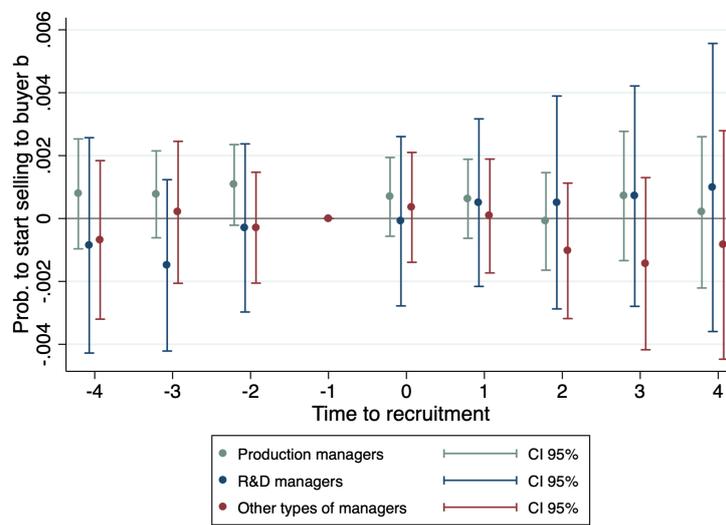


Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Equation (1.1), which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b k quarters before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times quarter, buyer \times quarter and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

Figure 1.8.10 – The effect of recruiting on the probability to start selling to a buyer - for other types of managers



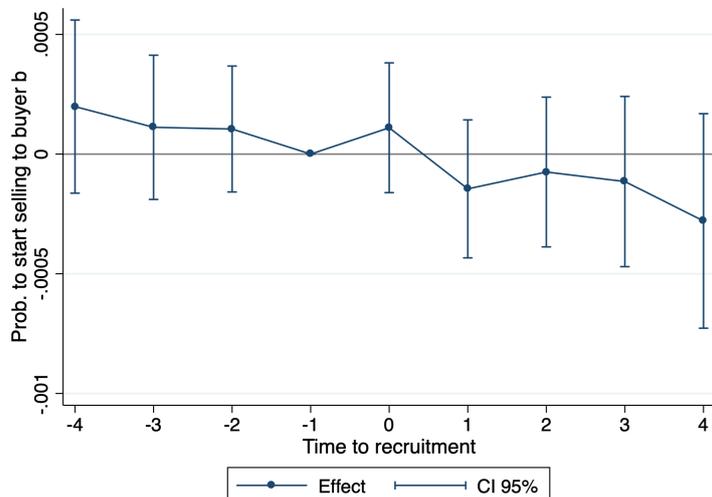
(a) Non-sales managers



(b) Type of non-sales managers

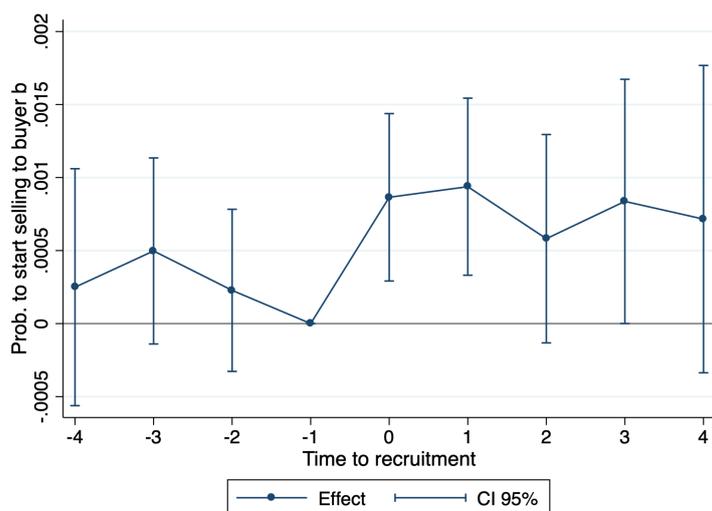
Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Eq 1.1, which correspond to the effect of recruiting a manager other than a sales manager from a firm selling to buyer b , k years before/after the recruitment. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

Figure 1.8.11 – The effect of recruiting a sales manager connected to a buyer on the probability to start selling to this buyer - falsification test with random recruitment years



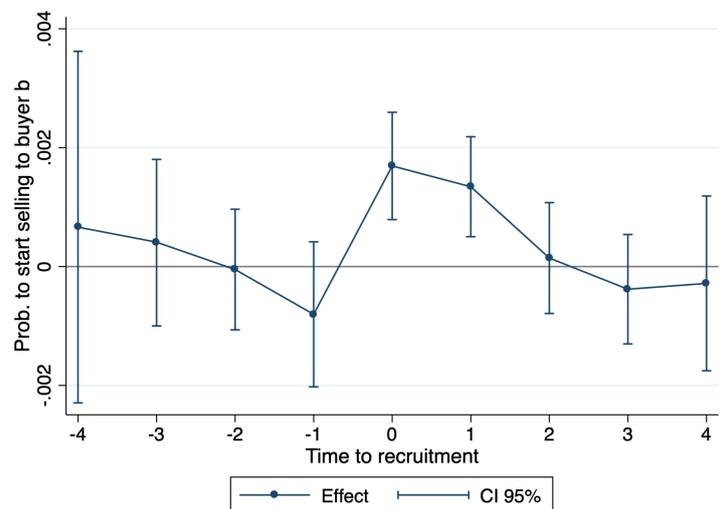
Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Equation (1.1), which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. We attribute random years of recruitment to each French firm \times foreign buyer pair as a falsification test. [Go back to main text](#)

Figure 1.8.12 – The effect of recruiting a sales manager connected to a buyer on the probability to start selling to this buyer - excluding firms in the same business group



Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Eq 1.1, which correspond to the effect of recruiting a worker from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. The sample excludes firms that recruit a sales manager from the same business group. [Go back to main text](#)

Figure 1.8.13 – The effect of recruiting a sales manager connected to a buyer on the probability to start selling to this buyer - using the De Chaisemartin-D’Haultfoeuille (2020) estimator



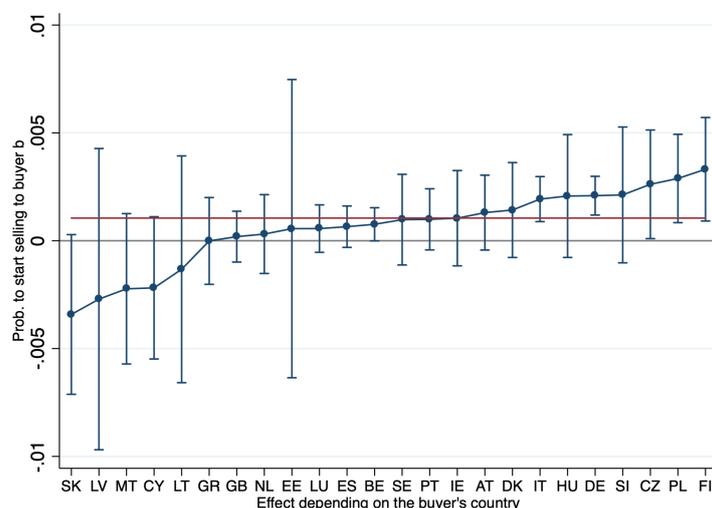
Notes: The graph displays the estimated β_k coefficients, $k \in [-4, 4]$, from Eq 1.1, which correspond to the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The estimation is performed using de Chaisemartin and d’Haultfoeuille (2020) estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

1.8.7 Appendix G: Additional heterogeneity tests

Countries Figure 1.8.14 shows the estimate from our event-study country by country. These are calculated using interaction terms rather than separate country regressions, in order to consistently estimate the firm \times buyer fixed effects. We find higher effects for Finland, Poland and the Czech Republic. Six countries attract a negative, but very imprecise, estimated coefficient.

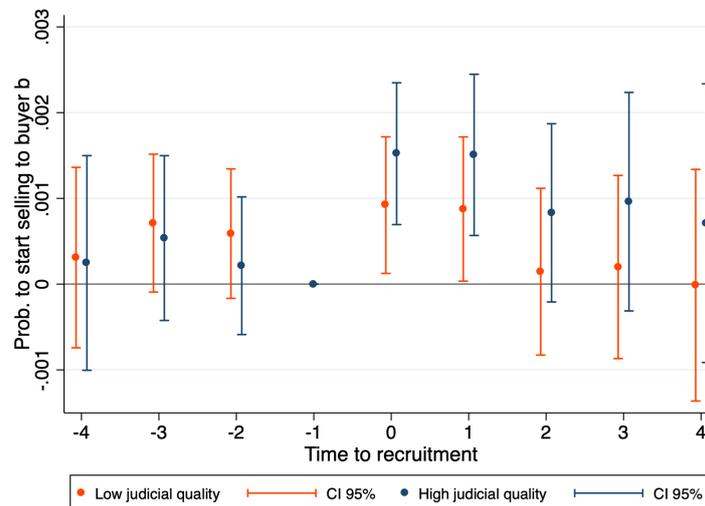
It is arguably easier for buyers to switch suppliers in protective legal environments. Greater judicial quality helps enforce contracts, and as a result foreign buyers may be less prone to change suppliers in risky legal environments. We test this hypothesis using the index of judicial-process quality from the World Bank. We carry out our main estimation separately for countries with low and high judicial-quality values, and find that recruiting a sales manager is more effective in high judicial-quality contexts. The results in Figure 1.8.15 show that the effect in the year of the recruitment is 1.7 times higher for buyers that are in countries with high judicial quality.

Figure 1.8.14 – The effect of recruiting on the probability to start to sell to a buyer - by country



Notes: The graph displays the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b the year of the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level.

Figure 1.8.15 – The effect of recruiting by country judicial quality



Notes: The graph displays the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. The buyer's country's judicial-process quality comes from World Bank Doing Business 2020. [Go back to main text](#)

Sales manager characteristics We split our main event-study according to some sales-manager observable characteristics. We first consider age: the results by age quartile appear in Figure 1.8.16a. The recruitment effect is significant only for sales managers of above-median age (over age 35); the effect then rises with age and is maximal for sales managers above age 43. To check that this does not reflect older sales managers being in more-qualified occupations, we control for the sales manager being highly qualified (highly-qualified sales managers are at hierarchy level 3). The results in Figure 1.8.16b reveal a qualitatively-unchanged pattern: even conditional on occupation, older sales managers have a greater recruitment effect.

We next consider the sales manager's wage. Figure 1.8.16c shows that higher-paid sales managers drive the results. It is of course true that there is a strong positive correlation between sales-manager age and wage.

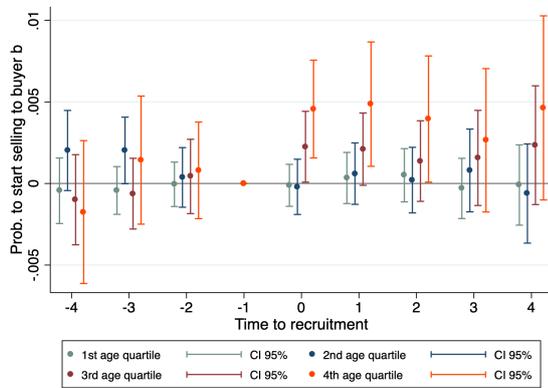
We also look at the wage difference of the sales manager between her two jobs. Figure 1.8.16d shows that the estimated recruitment effect is higher the larger the sales manager's wage rise when changing firms. This suggests that sales managers are rewarded for the buyers that they bring to their new firm.

Last, Figure 1.8.16f splits the effect by sales-manager gender: the recruitment effect is larger for women than for men.

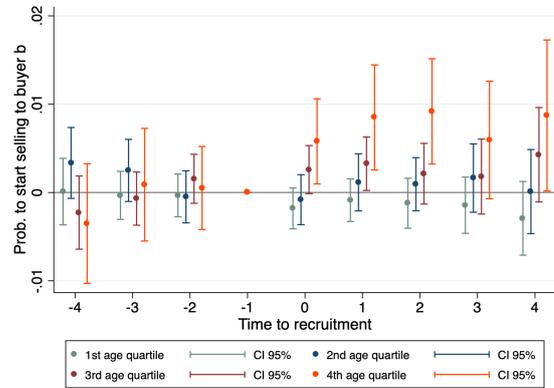
Buyer's characteristics Figure 1.8.17 considers a variety of buyer characteristics: the number of suppliers in panel (a), upstreamness in panel (b), the frequency of imports in panel (c) and (d), and the stickiness of the products imported in panel (e). The recruitment effect is higher for small buyers, downstream buyers, buyers importing infrequently from France, and buyers importing non-sticky products.

Firm characteristics Figure 1.8.18 shows that the recruitment effect is the same for wholesale and manufacturing recruiting or poached firms, but is larger for recruiting firms with fewer buyers and a smaller share of sales managers in their total employment. In Figure 1.8.18e, the effect is higher when the recruiting firm does not export at the beginning of the period, but remains significant for firms that do. Last, in Figure 1.8.18f the effect is higher when the recruiting firm does not export to the buyer's country at the beginning of the period.

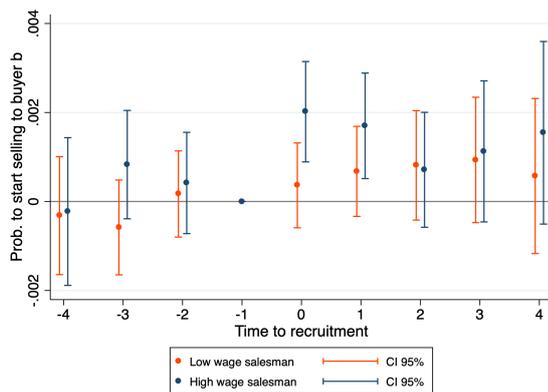
Figure 1.8.16 – The effect of recruiting on the probability to start selling to a buyer - by sales-manager characteristics



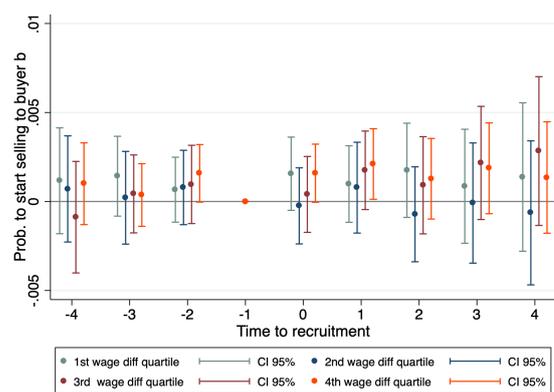
(a) Age



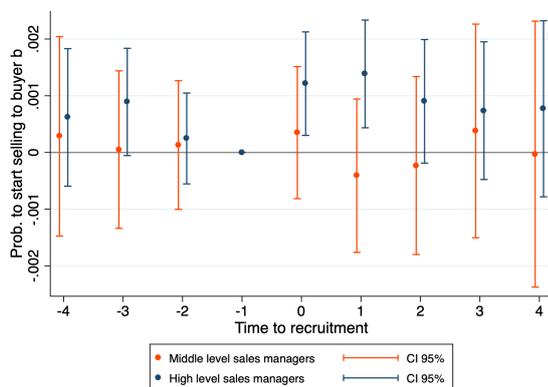
(b) Age controlling for qualifications



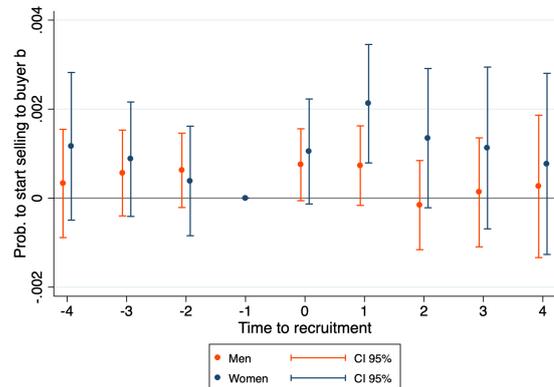
(c) Wage



(d) Wage difference btw the poached and recruiting firms



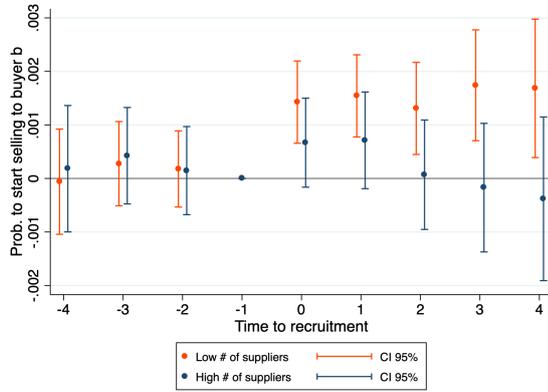
(e) Hierarchical level



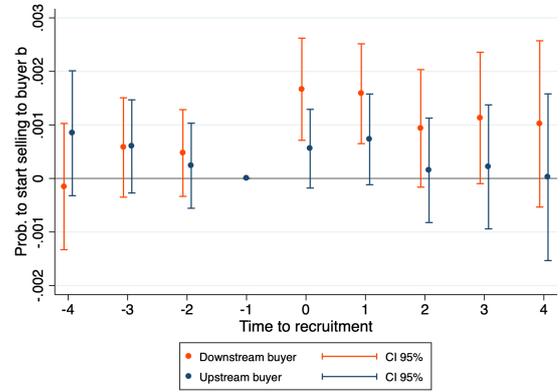
(f) Gender

Notes: The graphs display the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

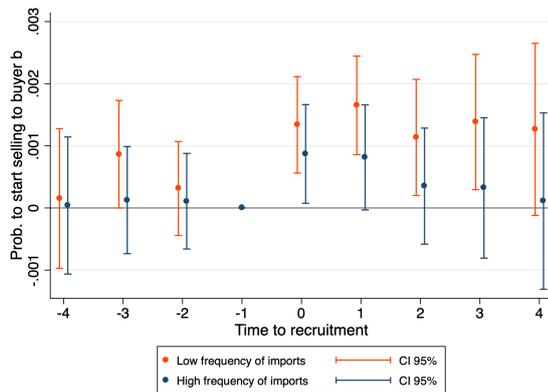
Figure 1.8.17 – The effect of recruiting on the probability to start selling to a buyer - by buyer characteristics



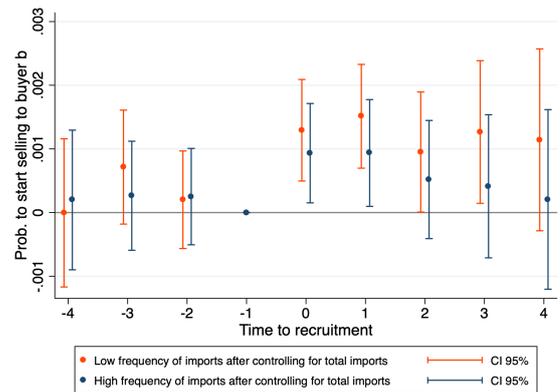
(a) Number of suppliers



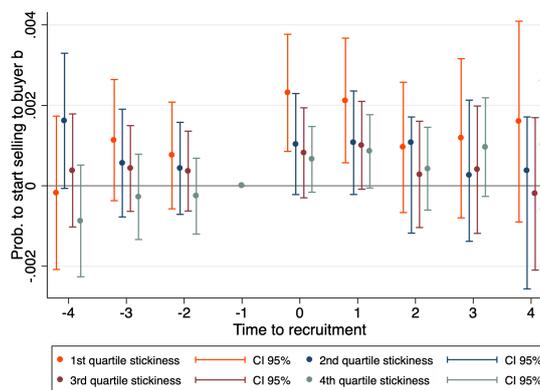
(b) Upstreamness



(c) Frequency of imports



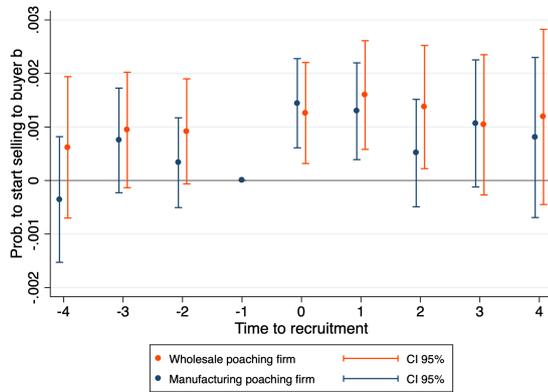
(d) Frequency of imports after controlling for imports



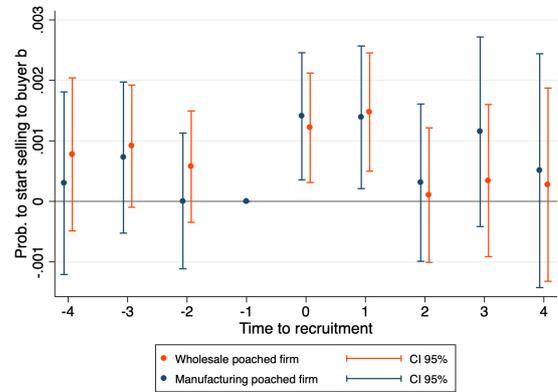
(e) Buyer's stickiness

Notes: The graphs display the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The estimation is carried out using the standard event-study estimator. A buyer's stickiness is defined with respect to the stickiness of the products imported by buyers, as defined in Martin et al. (2018). The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

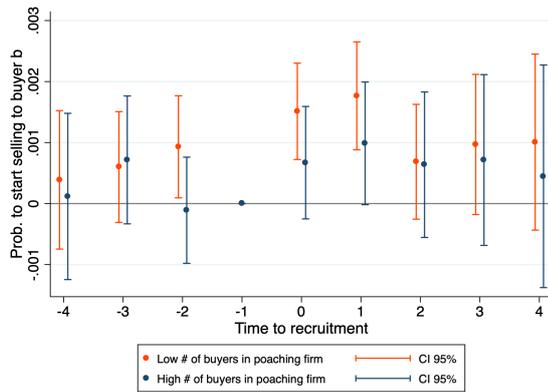
Figure 1.8.18 – The effect of recruitment by recruiting firm’s characteristics



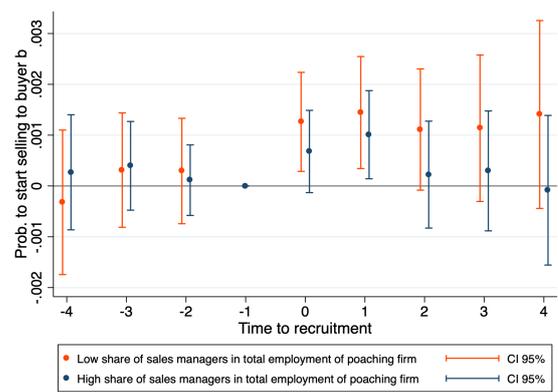
(a) Recruiting firm’s sector



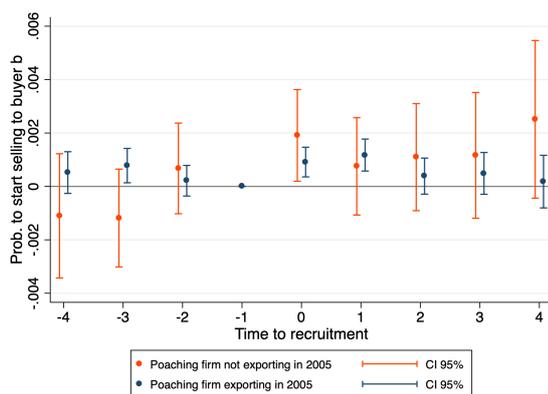
(b) Poached firm’s sector



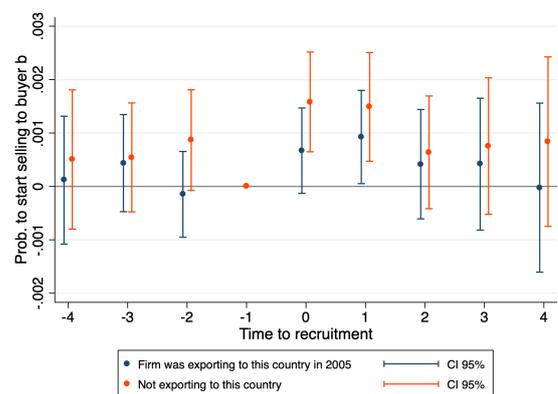
(c) Recruiting firm’s number of buyers



(d) Share of sales managers in recruiting firm’s employment



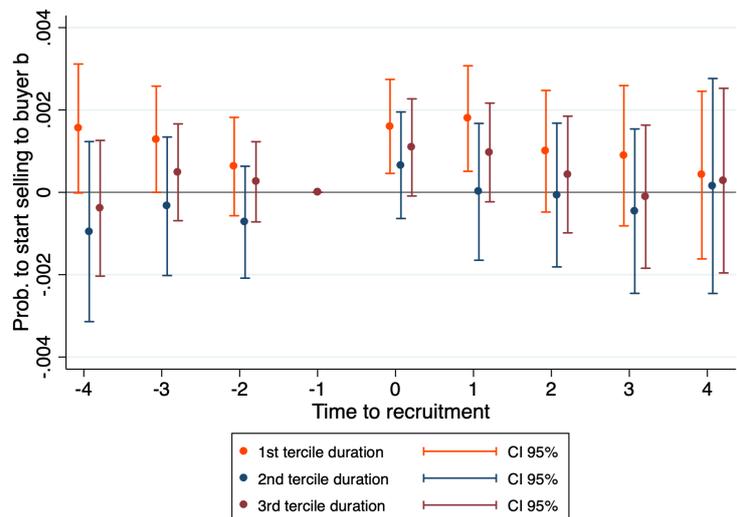
(e) Whether recruiting firm exports in 2005



(f) Whether recruiting firm exports to buyer’s country in 2005

Notes: The graph displays the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. [Go back to main text](#)

Figure 1.8.19 – The effect of recruitment by the duration of the relationship between the poached firm and buyer

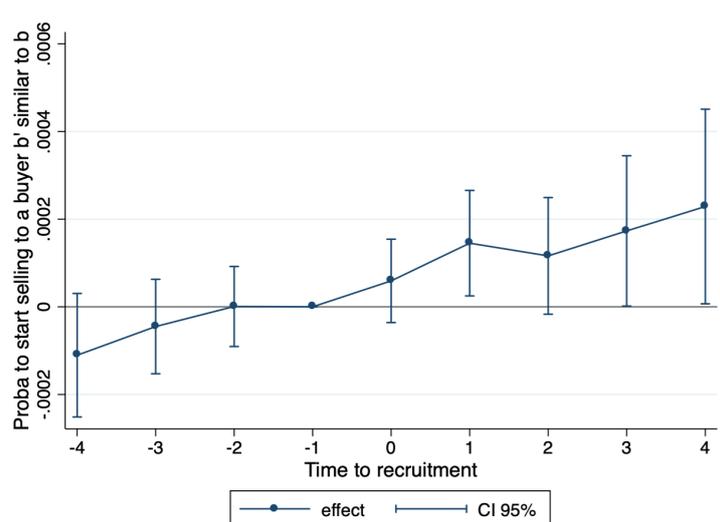


Notes: The graph displays the effect of recruiting a sales manager from a firm selling to buyer b on the probability to start selling to buyer b , k years before/after the recruitment. The estimation is carried out using the standard event-study estimator. The unit of analysis is the French firm \times foreign buyer pair. Firm \times year, buyer \times year and firm \times buyer fixed effects are included. 95% confidence intervals are displayed. Standard errors are clustered at the firm level. We show the effects according to the duration of the relationship between the poached firm and the buyer prior to sales-manager departure. The duration terciles correspond to relationships lasting under 1.25 years, 1.25 to 4 years, and 4 years or more. We display the effects according to whether the sales managers changed 2-digit sector when transitioning to her new firm. [Go back to main text](#)

1.8.8 Appendix H: Country-product knowledge

We here look at the effect for a firm of recruiting a sales manager connected to buyer b on the probability to start selling to buyer b' , where b' is a buyer that is very similar to buyer b . We take the 50-closest buyers to buyer b in terms of product, country and export volume, and re-estimate Equation (1.1) at the $fb't$ level. In this event-study, the reverse causality argument cannot hold: it seems unlikely that when a French firm f meets a foreign buyer b' , this buyer requires that the firm hires a sales manager that buyer b already knows in order to carry out the transaction. This complementary event-study estimate captures the ability of sales managers to bring buyers similar to those they already know to their firm, but does not reflect their buyer-specific knowledge. The results in Figure 1.8.20 reveal a significant, but smaller, effect, of recruiting on the probability to export to buyer b' . This confirms that sales managers have an exploratory role, and enable their firm to meet new buyers. This exploratory effect is 9% of that in our main event study, which is a lower figure than that found when we aggregate the results at the cell level, as in Section 1.6. We here restrict the analysis, for dimensionality reasons, to the 50-closest buyers to buyer b in terms of product, country and export volume. The difference across samples and the very high dimensionality of the data may explain this difference with respect to Section 1.6.

Figure 1.8.20 – The effect of recruiting a sales manager connected to buyer b on the probability to start selling to a buyer similar to b in terms of the product imported



Notes: The graph displays the effect for a firm of recruiting a sales manager connected to buyer b on the probability to start selling to buyer b' , where buyer b' is in the same country and 8-digit product category as b . We select the 50-closest buyers to b in the same country as b , in terms of 8-digit product imported and the volume of imports. [Go back to main text](#)

1.8.9 Appendix I: A theoretical model to quantify the business-network effect

This Section sets out a theoretical model that serves as a quantification device: the model helps disentangle the true business-network effect from a random-meeting effect. The model takes into account the potential heterogeneity in sales-manager ability. Overall, we find that our quantification described in Section 1.4.4 is not particularly sensitive to sales-manager heterogeneity.

Theoretical framework

Model settings Environment There are J countries. In each country, there are three types of agents: a mass of infinitely-lived selling firms, a mass of infinitely-lived sales managers, and a mass of finitely-lived buying firms. We assume that all three masses are exogenous. There is a unique final good that is traded in a frictional good market. We assume that in order to sell its final good, a selling firm needs to hire sales managers, who canvass buyers and build up a customer base. For selling firms, having more sales managers increases the meeting rate with buying firms. The modelling of the product market is otherwise very similar to Fontaine, Martin and Mejean (2020) and Eaton et al. (2016).

One crucial feature of our empirical analysis is what happens to relationships between buying firms and selling firms when a sales manager leaves a selling firm. Our model therefore needs to account for sales-manager transitions across firms. To do so, we model the labor market as in standard matching models, such as Postel-Vinay and Robin (2002).

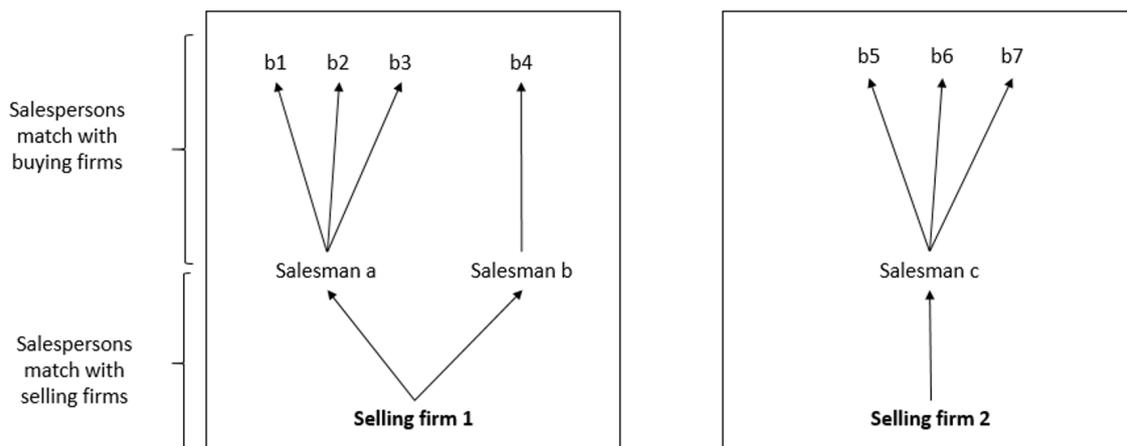
We therefore model two simultaneous matching processes: that between sales managers and selling firms, and that between sales managers and buying firms. Figure 1.8.21 sketches the matching process. Time is continuous, and we focus on steady-state outcomes.

Sales managers In each country j , there is a continuum of sales managers of measure $M_j z_m^{-\theta}$. We assume that sales managers cannot move across countries. Each sales manager draws an ability z from a Pareto distribution F with parameter θ and support $[z_m, +\infty[$. This ability z determines the meeting rate of the sales manager with buying firms: efficient sales managers meet buying firms more frequently.²⁶

Buying firms There is an exogenous mass B_i of homogeneous buyers in country i , each of

²⁶We assume that sales managers do not exert any effort: the meeting rate between a sales manager and buying firms is therefore exogenous, and depends on both the ability z of the sales manager and product-market frictions.

Figure 1.8.21 – Matching between sales managers, French firms and foreign buyers



Notes: Selling firms and sales managers match on the labor market. Sales managers and buying firms match: selling firms then sell only to the buying firms to which their sales managers have matched.

which buys from one selling firm only. In each period, an exogenous proportion α_i of buyers in country i exit the market and are replaced by new buyers. Buyers meet with employed sales managers in a frictional market, and choose to buy from the most-productive selling firm they meet. Sales managers serve only as meeting technologies between selling and buying firms. We assume that a buying firm meets a given sales manager with ability z in country j at rate $\gamma_{ij}(z)$, and that $\gamma_{ij}(z)$ increases in z . The rate at which a buying firm meets sales managers with ability z in country j is therefore $\gamma_{ij}(z)f(z)$, with $f(z)$ being the density of sales-manager ability. We denote $\gamma_{ij} = \int_{z_m}^{\infty} \gamma_{ij}(z)f(z)dz$. Thus γ_{ij} is the meeting rate between buyers in country i and sales managers in country j . The bilateral component reflects how the geographical and cultural distances between countries affect the matching process. We denote by $\gamma_i = \sum_{j=1}^J \gamma_{ij}$ the meeting rate between buyers in country i and sales managers in any country.

Selling firms There exists an infinitely-lived and exogenously-fixed mass $T_j \varphi_m^{-\eta}$ of firms in country j . Firms draw their productivity φ from a distribution $J(\varphi)$. The heterogeneity in the productivity dimension will determine the number of sales managers the firm is able to hire, and thus the number of buyers of the selling firm in equilibrium.

Matching between selling firms and sales managers Labor-market matching is largely inspired by the Postel-Vinay and Robin (2002) partial-equilibrium model in that sales managers can search on-the-job. In a given period, sales managers can be employed or unemployed within their labor market in country j . In every country, the job-offer arrival rate λ is exogenous, and a match between a worker and a firm is exogenously destroyed with probability δ ,

after which the worker becomes unemployed.²⁷

Matching between selling and buying firms The matching between selling and buying firms is indirect through the sales-manager technology. Sales managers meet buying firms in the product market, and buying firms choose to import from most productive of the sales managers' selling firms. In other words, the sales-manager's ability z determines the meeting rate between selling firms and buying firms, but conditionally on a sales manager and a buying firm meeting, the buying firm's choice of supplier depends only on the selling firm's productivity. This matching is one-to-many: a buyer can only have one supplier but a supplier may have several buyers.

When leaving a firm for another firm, a sales manager in country j is able to bring with her a fraction μ of her buyer portfolio, while her initial firm retains $1 - \mu$ of the manager's buyers. The parameter μ reflects the transferability of a sales manager's human capital: it is a reduced-form parameter that captures both that sales managers possess buyer-specific knowledge and legislative aspects.²⁸ If buyers are entirely tied to their sales manager then μ equals 1, while under a binding non-solicitation agreement (when leaving a firm with one's buyers is forbidden by law) μ would equal zero.²⁹

Steady-state results

Matching between sales managers and selling firms In the steady state, the flows of sales managers into unemployment must equal the flows out of unemployment. We denote by u_m the unemployment rate of sales managers, which is the same in every country by construction. This equality of flows condition is:

$$\delta(1 - u_m) = \lambda u_m$$

The steady-state unemployment rate of sales managers in each country is therefore $u_m = \frac{\delta}{\delta + \lambda}$. We denote by $G(\varphi)$ the proportion of employed sales managers who work for a selling firm

²⁷We do not allow for the meeting and separation rates λ and δ to vary across countries to keep the labor market part of the model as simple as possible.

²⁸A simple expression for μ could be :

$$\mu = \mathbb{1}(\varphi - \varphi' \geq c) \tag{1.4}$$

where c is the cost for a buyer to switch suppliers, which may depend on sectors and countries - , φ' is the productivity of the poached firm and φ that of the poaching firm. A buyer follows the current sales manager only if her new firm's productivity exceeds the sum of the current firm's productivity and the switching cost. However, we do not choose to take a stance on the functional form of this μ , and keep it as a reduced-form object.

²⁹We abstract from modeling the ability of firms and workers to sign non-compete agreements, as analyzed in Shi (2018).

with productivity below φ . At the steady state, the inflows of sales managers into firms with productivity below φ must equal the outflows, so that:³⁰

$$\lambda u_m J(\varphi) = [\delta + \lambda(1 - J(\varphi))] G(\varphi)(1 - u_m) \quad (1.5)$$

Inflows correspond to unemployed sales managers who match with firms with a productivity below φ . Outflows correspond to the exogenous destruction of matches between sales managers and selling firms, as well as sales managers matched with selling firms with productivity below φ who meet a selling firm with productivity above φ (which occurs at rate $\lambda(1 - J(\varphi))$). We denote $\bar{G} = 1 - G$ and $\bar{J} = 1 - J$. Equation (1.5) immediately yields:³¹

$$G(\varphi) = \frac{\delta J(\varphi)}{\delta + \lambda \bar{J}(\varphi)} \quad (1.6)$$

Matching between buying and selling firms We denote by $H_i(\varphi)$ the fraction of buyers in country i who are matched to a firm with efficiency below φ . This fraction is stable in the steady state. The flow equation for buying firms is:

$$u_{ib} \times \mathbb{P}(\text{buyer meets a supplier with productivity} \leq \varphi) = (1 - u_{ib})H_i(\varphi) \times [\alpha_i + \mu\lambda\bar{J}(\varphi) + \mathbb{P}(\text{buyer meets a supplier with productivity} \geq \varphi)]$$

where u_{ib} is the proportion of buyers in country i that are unmatched. Inflows correspond to unmatched buyers in country i that meet a selling firm with productivity below φ . Outflows correspond to three types of buyers. First, buyers matched to selling firms with productivity below φ and whose match is exogenously destroyed, which occurs at rate α_i . Second, buyers matched to selling firms with productivity below φ and whose sales manager moves to a new firm above φ , which occurs at rate $\lambda\bar{J}(\varphi)$, and whose sales manager brings the buyer to her new firm, which occurs at rate μ . Third, buyers matched to selling firms with productivity below φ and which meet a better firm - *i.e.* a supplier with productivity above φ .³²

The meeting rate between a buying firm and a selling firm depends on both the number and ability of sales managers in the selling firm. In the steady state, the probability that a

³⁰We assume that matching is random, so all firms have the same probability of being sampled. As a result, the sampling distribution of offers corresponds to the distribution of firm productivities.

³¹As our main focus is on product-market matching, we abstract from modeling differences in sales manager-selling firm matching across countries: by construction, the proportion $G(\varphi)$ of employed sales managers who work for a firm with productivity below φ is the same across countries.

³²At the steady state, the flows of buyers becoming unmatched must equal the flows of unmatched buyers that become matched. This equality of flows condition is $\alpha_i(1 - u_{ib}) = \gamma_i u_{ib}$, and immediately yields $u_{ib} = \frac{\alpha_i}{\alpha_i + \gamma_i}$. We assume that when a sales manager enters unemployment, her (selling) firm retains all of her buyers.

buyer meet a supplier in country j with productivity below φ is simply the probability γ_{ij} of meeting a selling firm in country j multiplied by the proportion of employed sales managers working for a selling firm with productivity below φ . As a result:

$$\begin{cases} \mathbb{P}(\text{buyer meets a supplier in country } j \text{ with productivity } \leq \varphi) = \gamma_{ij}G(\varphi)(1 - u_m) \\ \mathbb{P}(\text{buyer meets a supplier with productivity } \geq \varphi) = \gamma_i\bar{G}(\varphi)(1 - u_m) \end{cases}$$

We obtain:

$$H_i(\varphi) = \frac{\alpha_i G(\varphi)(1 - u_m)}{\alpha_i + \lambda\mu\bar{J}(\varphi) + \gamma_i\bar{G}(\varphi)(1 - u_m)}$$

Business-network effect When a sales manager leaves selling firm φ' for selling firm φ , she brings a former client of hers at the following rate p_1 :

$$p_1 = \mu[1 - \gamma_i\bar{G}(\varphi)(1 - u_m)]$$

The buyer is transmitted by the sales manager to her new firm if the buying firm is transferable, which occurs at rate μ , and if the buying firm does not meet a better selling firm, which occurs at rate $(1 - \gamma_i\bar{G}(\varphi)(1 - u_m))$, at the same time as the sales manager's job-to-job transition. The business-network effect falls in γ_i , and rises in the poaching-firm's productivity φ and the transferability μ of business networks.

Random-meeting effect A sales manager may meet a buying firm without having interacted with it in the past. As a result, when a sales manager leaves a selling firm, she may meet a buyer of her former firm randomly, even if she had no previous personal relationship with this buyer. For this to occur, the sales manager z must meet the buying firm, which occurs at rate $\frac{\gamma_{ij}(z)B_i}{M_j z_m^{-\theta}}$, and the sales-manager's new firm must be more productive than the current supplier of the buying firm, which occurs with probability $H_i(\varphi)$.³³ The rate p_2 is thus:

$$p_2 = \frac{\gamma_{ij}(z)B_i}{M_j z_m^{-\theta}} H_i(\varphi)$$

The random-meeting effect p_2 rises in $\gamma_{ij}(z)$, falls in γ_i (through the equilibrium value of $H_i(\varphi)$) and rises in the poaching-firm's productivity φ .

Event-study estimand The goal of the model is to give a structural interpretation to

³³A buyer in country i meets with a sales manager with ability z from country j at a rate of $\gamma_{ij}(z)f(z)$. Therefore, consistency implies that, from the point of view of a sales manager z in country j , the probability to meet a buyer in country i is $\frac{\gamma_{ij}(z)f(z)B_i}{M_j z_m^{-\theta} f(z)} = \frac{\gamma_{ij}(z)B_i}{M_j z_m^{-\theta}}$.

our event-study estimand in order to recover the true business-network effect. We obtain that the event-study estimand is:

$$\begin{aligned} \beta(z, \varphi, \varphi') = & \mathbb{P}(\text{poached sales manager interacted with } b) \times \underbrace{\mu[1 - \gamma_i \bar{G}(\varphi)(1 - u_m)]}_{p1 = \text{business-network effect}} \\ & + (1 - \mathbb{P}(\text{poached sales manager interacted with } b)) \times \underbrace{\gamma_{ij}(z) \frac{H_i(\varphi) B_i}{M_j z_m^{-\theta}}}_{p2 = \text{random-meeting effect}} \end{aligned} \quad (1.7)$$

with z the ability of the sales manager, φ' the productivity of the poached firm and φ the productivity of the poaching firm. The event-study estimand is therefore a weighted average of business-network transmission and the general capacity of a sales manager to canvass buyers. The *absolute* importance of business-network transmission is p_1 in the above equation: this corresponds to the probability that a sales manager sell to a buyer with whom she was connected previously. The *relative* importance of business-network transmission relative to the general capacity of a sales manager to bring in a buyer is $\frac{p_1}{p_2}$. This corresponds to the rise in the probability for a sales manager to sell to a buyer when she was previously connected to the buyer relative to the case where she was not.

Our goal is to recover the business-network effect. This depends on the functional form of $\gamma_{ij}(z)$. We assume that, for a buyer in country i , the meeting rate with a sales manager with ability z in country j is:

$$\gamma_{ij}(z) = \gamma_{ij} \frac{\kappa + \theta}{\kappa} (1 - z_m^\kappa z^{-\kappa}) \quad (1.8)$$

with $\kappa \in]0, \infty[$. This functional form mirrors the Pareto distribution from which sales managers' abilities are drawn. However, we allow for the heterogeneity of meeting rates between sales managers and buyers (determined by κ) to differ from the heterogeneity in sales managers' ability (determined by θ). This functional form entails that, no matter κ , $\mathbb{E}[\gamma_{ij}(z)] = \gamma_{ij}$. κ thus characterizes the heterogeneity of meeting rates across sales managers, without affecting the average meeting rate γ_{ij} between a sales manager in country j and a buyer in country i . The higher is κ , the lower is this heterogeneity.

From this model, we can write the probability that the poached sales manager was the sales manager dealing with buyer b in her previous firm as:

$$\omega = \mathbb{P}(\text{poached sales manager interacted with } b) = \frac{1}{l(\varphi')} \frac{\kappa + \theta}{\kappa} (1 - z_m^\kappa z^{-\kappa}) \quad (1.9)$$

where $l(\varphi')$ is the number of sales managers in the poached firm φ' . Substituting into Equation (1.7) and taking the expectation over z gives:

$$\beta(\varphi, \varphi') = \frac{1}{l(\varphi')} \times \underbrace{\left(\mu[1 - \gamma_i \bar{G}(\varphi)(1 - u_m)] - \frac{2(\kappa + \theta)}{2\kappa + \theta} \frac{\gamma_{ij} H_i(\varphi) B_i}{M_j z_m^{-\theta}} \right)}_{e_1} + \underbrace{\frac{\gamma_{ij} H_i(\varphi) B_i}{M_j z_m^{-\theta}}}_{e_2} \quad (1.10)$$

In the data, we observe the number of sales managers $l(\varphi')$ in the poached firm. Interacting the event-study estimate with $1/l(\varphi')$ enables us to estimate \hat{e}_1 and \hat{e}_2 , and therefore the business-network effect as:

$$\hat{p}_1 = \hat{e}_1 + \frac{2\kappa + 2\theta}{2\kappa + \theta} \hat{e}_2$$

We then retrieve the ratio $\frac{\hat{p}_1}{\hat{p}_2}$ in order to conclude: for a sales manager, having interacted with a particular buyer b increases the probability to sell to that buyer by a factor $\frac{\hat{p}_1}{\hat{p}_2}$. \hat{p}_1 falls in κ : the more heterogeneous the meeting rates of sales managers across sales managers (the lower is κ), the greater the business-network effect.

Results: quantification of business networks

Main results We estimate that $\hat{e}_1 = 0.0062614$ and $\hat{e}_2 = 0.0005997$. The model tells us how to recover the extent of the business-network effect, depending on the values of the parameter κ . Under the extreme assumption that all sales managers have the same meeting rate with buyers ($\kappa = \infty$), we find $\hat{p}_1 = .0068611$. This implies that for a sales manager, having interacted with a particular buyer b increases the probability to sell to that buyer by a factor of 11.4. Under the more-plausible assumption that meeting rates are heterogeneous and that $\kappa = \theta$, *i.e.* the heterogeneity of the meeting rates of sales managers equals that of sales-manager ability, we obtain $\hat{p}_1 = .007061$. Under this assumption, having interacted with a particular buyer b increases the probability to sell to that buyer by a factor of 11.8.

Extensions What if the transferability μ of buyers across selling firms rises in sales-manager ability z ? It could be that efficient sales managers transfer their former clients to their new firm more easily. If we specify $\mu(z) = \mu \frac{\nu + \theta}{\nu} (1 - z_m^{-\nu} z^{-\nu})$, with $\nu \in]0, \infty[$, then

Equation (1.10) becomes:

$$\beta(\varphi, \varphi') = \frac{1}{l(\varphi')} \times \underbrace{\left(\frac{\kappa + \nu + 2\theta}{\kappa + \nu + \theta} \mu [1 - \gamma_i \bar{G}(\varphi)(1 - u_m)] - \frac{2(\kappa + \theta)}{2\kappa + \theta} \frac{\gamma_{ij} H_i(\varphi)}{M_j z_m^{-\theta}} \right)}_{e_1} + \underbrace{\frac{\gamma_{ij} H_i(\varphi)}{M_j z_m^{-\theta}}}_{e_2} \quad (1.11)$$

To obtain our previous results, we assumed that $\nu = \infty$, so that there was no heterogeneity in $\mu(z)$ across sales managers. Table 1.8.4 explores the sensitivity of the quantification to different values of κ , which determines the heterogeneity in the meeting rate with buyers across sales managers, and ν , which determines the heterogeneity in the transferability of buyers across sales managers. Our preferred estimate is that having interacted with a particular buyer b increases the probability to sell to that buyer by a factor of 8.7: this results from the assumption that $\nu = \kappa = \theta$, which seems the most reasonable one when we do not know the true values of ν and κ . Table 1.8.4 provides the lower and upper bounds for the quantification: these are respectively 6.1 and 12.3.

Table 1.8.4 – The sensitivity of the quantification to different heterogeneity parameters

	$\kappa \rightarrow 0$	$\kappa = \theta/2$	$\kappa = \theta$	$\kappa = 2\theta$	$\kappa \rightarrow \infty$
$\nu \rightarrow 0$	6.14	7.06	7.74	8.61	11.27
$\nu = \theta/2$	7.36	7.85	8.29	8.92	11.27
$\nu = \theta$	8.18	8.41	8.71	9.18	11.27
$\nu = 2\theta$	9.21	9.16	9.29	9.56	11.27
$\nu \rightarrow \infty$	12.27	11.77	11.61	11.47	11.27

Notes: Our theoretical framework in Section 1.8.9 enables us to answer the following question: for a sales manager, how much does the probability to sell to a buyer increase when she has interacted with the buyer before? The answer to this question depends on three model parameters: θ , which determines the heterogeneity in sales managers' ability, κ , which determines the heterogeneity in the meeting rate with buyers across sales managers, and ν , which determines the heterogeneity in the transferability of buyers across sales managers. The higher these three, the less heterogeneity there is across sales managers. The table reads as follows: if $\kappa = \nu = \theta$, then having interacted with a particular buyer b increases the probability to sell to that buyer by a factor of 8.7. The table gives lower and upper bounds for this quantification: these are 6.1 and 12.3 respectively.

Chapter 2

Judge Bias in Labor Courts and Firm Performance

This chapter is co-authored with Pierre Cahuc (Sciences Po), Stéphane Carcillo (OECD) and Flavien Moreau (IMF).¹

Abstract Does labor court uncertainty and judge subjectivity influence firms performance? We study the economic consequences of judge decisions by collecting information on more than 145,000 Appeal court rulings, combined with administrative firm-level records covering the whole universe of French firms. The quasi-random assignment of judges to cases reveals that judge bias, defined as judge-specific differences on qualifying dismissals as wrongful and granting compensation, has statistically significant effects on the survival, employment, and sales of small firms, especially among very small and low-performing ones. When compensation for wrongful dismissal is instrumented by judge bias, an increase in compensation of 1 percent of the payroll reduces employment by 3 percent after 3 years for those firms. However, we find that the uncertainty associated with the actual dispersion of judge bias is small and has a non-significant impact on their average outcomes.

Keywords: Dismissal compensation, judge bias, firm survival, employment.

JEL classification: J33, J63, J65.

¹I thank the *Chaire Sécurisation des Parcours Professionnels* for its financial support. I thank Camille Hebert for providing invaluable help in retrieving firm identifiers. I also thank Andrea Ichino, Julien Sauvagnat, Yanos Zylberberg and seminar participants at CREST and Sciences Po for their comments. The views expressed herein are those of the authors and should not be attributed to the IMF, its Executive Board, or its management. This chapter has benefited from the IAAE travel grant for the 2019 IAAE Conference in Nicosia. This work is supported by a public grant overseen by the French National Research Agency (ANR) as part of the 'Investissements d'avenir' program (reference : ANR-10-EQPX-17 - Centre d'accès sécurisé aux données - CASD).

2.1 Introduction

Outcome unpredictability, the fear of a differentiated treatment, and judges' alleged pro-worker biases are frequent worries of businesses heading to labor court. Recently, many advanced economies have therefore enacted reforms that restrict judge latitude in awarding compensations, with the objective of limiting economic uncertainty and guarding businesses against dramatic outcomes.² However, none of these regulations has been grounded on rigorous quantitative analysis, partly for lack of appropriate data.

This chapter therefore presents the first systematic evidence of the impact of labor court judge bias on firms economic performance. We use text analysis to extract rich information from about 145,000 decisions made by French Appeals court over the period 2006-2016. This allows us to identify judge bias – defined as the effects of judge-specific differences on compensations for wrongful dismissals – from the quasi-random allocation of cases to judges.

We find evidence that some judges are more pro-worker than others, meaning that conditional on observables, they are more likely to i) consider more often that dismissals are wrongful and ii) set higher compensation levels conditional on characteristics of cases. The difference between the compensation set by the most pro-worker and the most pro-employer judges is significant: moving from the bottom decile to the top decile of judge bias increases expected compensation payments by about two months of salary, or 20 percent of the average compensation.

We then explore the impact of judge bias on firm performance, drawing on administrative firm-level records covering the whole universe of French firms. Given that these cases overwhelmingly concern the dismissal of a single employee at a time,³ we focus on small firms – below 100 employees at the date of the judgment – for which the magnitude of the associated cashflow shocks are more likely to matter, and which therefore provide an upper-bound of the uncertainty effect we seek to estimate. From reduced-form regressions, we find that the judge bias has in fact a significant impact on firm survival, sales and employment, but only for those firms that are very small – 10 employees or less – and low-performing – return on assets below the median. There are no significant effects for the other firms. From instrumental variable

²In Italy, the 2014 Jobs Act, the Renzi cabinet's main labor reform, aimed at reducing uncertainty due to excessive litigation and the unpredictability of judges' decisions (Boeri and Garibaldi (2018)). Similarly in France, the 2017 Ordonnances reforming the labor code introduced a ceiling to the level of compensation granted by judges, based on firm size and worker seniority. In a majority of European countries, judges' discretion in compensating the individual damages following wrongful dismissals is actually capped (see Annex A). In the U.S., employment protection is overseen by the National Labor Relation Board (NLRB) whose judges have been denounced by some critiques as being influenced by partisan ideology (Turner (2006), Semet (2016)).

³The small number of collective layoffs does not provide sufficient observation to proceed to a quantitative analysis

regressions, in which the compensation for wrongful dismissal is instrumented by the judge bias, we find that an increase in the amount of compensation of 1 percent of the payroll of the firm reduces employment at 3 years horizon by 3 percent for firms whose returns on assets is below the median, but has no employment effects for other firms.

We pay special attention to establishing the credibility of our identification strategy, which is supported by several key institutional features. Appeals cases for wrongful dismissal are decided by three-judge panels composed of a president and their two assessors in a section of the court called “social chamber”. We focus on the presidents, who oversee all the rulings and accordingly play a key role in deciding the case, and leverage their rotations across courts. To identify the effects of judge-specific differences on compensations for wrongful dismissals, we compare the compensations decided by subsequent presidents of social chambers within the same social chamber of the same Appeal court within the same year. More precisely, we estimate for each judgment the president bias using a leave-one-out difference between the average compensations for all other cases that a president has handled and the average compensations handled in the same social chamber within the same year by all presidents.

In order to document the random allocation of cases to judges, we first perform an event study to verify whether judges of different types judge firms with similar performance before the judgment. In particular, we compare total, permanent and temporary⁴ employment growth relative to the year preceding the judgment for two groups of firms : (i) firms which face a pro-worker judge, whose bias is above the median and (ii) firms which face a pro-employer judge, whose bias is below the median. We find that employment growth is not statistically different between those two groups of firms before the judgment. However, total and permanent employment start diverging after the judgement, especially among small and low performing firms. Eventually, we verify that the allocation of judges is unrelated to the observable worker and firm characteristics of the cases they judge. We therefore interpret the differences between leave-one-out mean compensations set by subsequent judges in the same social chamber of the same Appeal court in a given year as reflecting the influence of judges’ subjectivity.

What would be the impact of eliminating the *dispersion* of judge bias on firms’ *average* outcomes? To answer this question, we consider three thought experiments, assuming either i) that the bias of all judges is set to zero, or ii) that the bias of pro-worker judges is set to zero while that of pro-employer judges is unchanged, or iii) that the bias of all pro-employer judges is set to zero while that of pro-worker judges is unchanged. In each case we find small

⁴We count as permanent employment the employees hired on open-ended contracts or CDI (*Contrat a durée indéterminée*), as opposed to CDD (*Contrat a durée déterminée*).

statistically non-significant effects even for small, low-performing firms. Hence, we conclude that the actual dispersion of judge bias has no significant effects on the average performance of firms in our context. An important open question that our study cannot address, however, is the possibility that all judges are biased, meaning that setting all biases to the mean does not ensure the absence of bias of all judges in the interpretation of labor laws (see Ash, Chen and Naidu (2018)).

Our results should be interpreted cautiously as the dispersion of judge biases is itself an endogenous object, which may influence the decisions of firms and workers to go to court, even if the matching between judges and cases were random. More uncertainty about judge decisions likely induces more workers and employers to go to court, raising the litigation rate, and alters the composition of the set of cases going to litigation (see Priest and Klein (1984) and Lee and Klerman (2016)). To assess the strength of these potential selection effects, we compute the firm's risk premium associated with the dispersion in outcomes arising solely from the judge bias. Reassuringly, we find that the effect of the dispersion of judge bias on the selection of cases that go to Appeal courts is likely negligible, since the associated risk premium is at most equal to 1.5 percent of the expected amount of compensation conditional on observable worker and firm characteristics.

This chapter makes three important contributions to the literature. First, we provide the first direct estimate of labor court judge bias on dismissal compensation, thanks to a novel dataset with detailed, case-by-case information about compensation for wrongful dismissal. Differentiated treatment by judges has been investigated in a rapidly growing and influential empirical literature, in particular regarding criminal sentencing (Scott (2010), Dobbie, Goldin and Yang (2018), Yang (2015), Bhuller, Dahl and Mogstad (2020)), bankruptcies (Bernstein, Colonnelli, Giroud and Iverson (2018b), Bernstein, Colonnelli and Iverson (2018a)), or decisions related to disability benefits (Autor, Maestas, Mullen and Strand (2015), Dahl, Kostøl and Mogstad (2014), French and Song (2014), Kostøl, Mogstad, Setzler et al. (2017), Maestas, Mullen and Strand (2013), Autor, Kostøl, Mogstad and Setzler (2019)). Relying on the quasi-random or random allocation of judges to cases, these contributions generally find that differentiated treatment by judges is significant, but, importantly, that it can be mitigated by sentencing guidelines (Scott (2010), Yang (2015), Cohen and Yang (2019)). Bamieh (2016) uses this approach to infer firing cost variations from the dispersion in trial duration in Italian labor courts driven by quasi-random judge appointments. Semet (2016) finds that the propensity to reach a decision favoring labor increases with each additional Democrat judge added to a panel of the US *National Labor Relation Board*. Our main addition to this literature is to establish the differentiated treatment by judges both on the qualification of dismissals

and, crucially, on the *amounts* of compensation themselves, when the dismissal is deemed wrongful. Our two measures of judge bias are in line with previous research studying the impact of extraneous factors on the qualification of dismissals as unfair by judges. Ichino, Polo and Rettore (2003), Marinescu (2011) and Jimeno, Martínez-Matute and Mora-Sanguinetti (2020) show that the local unemployment and bankruptcy rates influence the probability that judges deem dismissals unfair. Consistent with these contributions, our findings show that judges retain some degree of autonomy in their interpretation of labor laws.⁵

Second, the chapter establishes the causal impact of dismissal costs *surprises* on firm performance, thanks to the merging of data on compensation for wrongful dismissal with administrative firm-level records. A vast empirical literature analyzes the labor market impact of dismissal costs (see Cahuc et al. (2014) for a survey) but causal evidence has mostly hinged on aggregate exogenous variations. In particular, studies of the effects of court decisions regarding unfair dismissals on firms' outcomes (Autor (2003), Autor, Donohue III and Schwab (2006), Autor, Kerr and Kugler (2007), Boeri and Garibaldi (2018), Fraise, Kramarz and Prost (2015), Gianfreda and Vallanti (2017), Martins (2009)) typically use the implementation of reforms of Employment Protection Legislation to assess the effects of dismissal costs on employment or productivity. Autor et al. (2007) use the adoption of wrongful discharge protections by U.S. State courts and find that higher employment protection leads to lower employment flows, lower firm entry rates and lower total factor productivity. In France, Fraise et al. (2015) use an instrumental strategy based on shocks to the supply of lawyers and infer that an increase in dismissal costs leads to a decline in employment fluctuations. Closest to our paper is Bamieh (2016) who shows that longer trials induced by specific differences in judges randomly assigned to firms reduce the labor turnover and increase employment in Italy. In contrast, our paper differs from previous studies in several crucial aspects. In the first place, we analyze the impact of the differentiated treatment by judges concerning the qualification of dismissals and the compensation for wrongful dismissal on firm performance. This is the first contribution exploiting such information at the firm level. In addition, this allows us to identify, for the first time, the causal impact of surprises on dismissal costs on firm performance. This is a key point insofar as the uncertainty associated with dismissal compensations is considered to be a major feature of employment protection legislation in European countries (Ichino et al. (2003), Marinescu (2011), Berger and Neugart (2011), Martín-Roman, Moral and Martínez-

⁵This is also consistent with Jimeno et al. (2020)'s study of Spanish labor reforms of 2010 and 2012. Despite a broadening of the definition of fair economic dismissals, the proportion of economic redundancies being ruled as fair by labor courts has not substantially increased. This discrepancy between the evolution of the legal rules and the "effective" rules is interpreted as arising from the opposition of judges to the change in the legal definition of fair dismissals, suggesting that judges have significant margin for interpreting legal rules.

Matute (2015), Jimeno et al. (2020) and in the US Posner (2008)). Besides, our contribution looks at the impact of employment protection legislation on the survival of small firms, an issue which has been overlooked by the literature so far. We find statistically significant effects of shocks on compensation for wrongful dismissal on employment and sales for firms whose returns on assets is below the median. From this perspective, it is related to the corporate finance literature that assesses the effect of exogenous cash flow and credit shocks, positive or negative, on firms (Blanchard et al. (1994), Chodorow-Reich (2013), Giroud and Mueller (2017), Rauh (2006)) and the effects of labor market regulation on access to credit (Simintzi, Vig and Volpin (2014), Favilukis, Lin and Zhao (2020)).

Third, we investigate the potential *uncertainty* effect of reducing the dispersion of judge bias on average firm performance, a main motivation behind several recent labor market reforms in Europe. We are not aware of any contribution shedding light on this issue. Our findings indicate that although the bias of some judges does have a significant impact on the performance of small, low-performing firms, the dispersion of bias is too limited to have significant effects on the average performance of those firms. This finding is striking to the extent that recent reforms have been implemented in 2017 to reduce the supposedly dispersion of judge bias in France, while our findings indicate that this dispersion has negligible effects over the 2006-2016 period.

The chapter is organized as follows. Section 2.2 describes the French institutional setting and Section 2.3 the data. Section 2.4 presents evidence about judge bias. Section 2.5 documents the impact of judge bias on firm survival and employment. Section 2.6 concludes.

2.2 Institutional background

This section starts by presenting the regulation of termination of open-ended employment contracts, which represent about 85 percent of ongoing contracts in France, before providing an overview of the organization of courts and describing the assignment of judges to cases.

2.2.1 Legal framework

Following the termination of an open-ended contract, employees with a tenure longer than one year and who did not commit any serious or gross misconduct (*faute grave* or *faute lourde*) are granted a minimum legal severance payment calculated as one fifth of monthly salary per year of tenure, plus an additional two fifteenths after ten-year tenure. These amounts can be topped up if the professional branch to which the firm belongs has signed a collective agreement setting higher payouts.

Under French law, terminations of open-ended employment contracts are lawful if they are justified by a 'real and serious cause', either economic or personal. Dismissals for economic reasons are lawful only to 'safeguard' firms, but not to improve their profitability. Dismissals for personal reasons are lawful only in case of misconduct or lack of adaptation to the job. For both types of dismissal, the burden the proof is on the side of employers. Furthermore, employers have to prove that there is no other position available in the firm (worldwide in the period we are studying) for dismissed employees when the dismissal is motivated by economic reasons or by lack of adaptation to the job.

When the employee deems her dismissal wrongful, she can file a complaint before the *Prud'hommes* councils, which are courts of first instance. While most European countries have specialized labor tribunals to deal with dismissal cases (see OECD (2013)), in France judges in *Prud'hommes* councils are employee and employer representatives, with an exact equality between the numbers of councilors representing employers and those representing employees.

Serverin and Valentin (2009) calculate that for economic dismissals in 2006, the rate of employee recourse to *Prud'hommes* in case of dismissal is between 1% and 2% while for disciplinary dismissals it is between 17% and 25%.⁶ According to Desrieux and Espinosa (2019), among claims that reached the judicial stage at *Prud'hommes* council from 1998 to 2012, 62%

⁶Economic dismissals are therefore very rarely challenged, one reason being that their conditions are usually negotiated between social partners at the firm level. Another reason is that these layoffs only account for 2% of all exiters, since employers prefer to have recourse to personal motives given the complexity of their procedure (when more than one person is laid off) and the absence of a legal or conventional definition of a lawful separation for economic reason (at least until a 2016 law which clarified this notion).

resulted in the acceptance of the employee's claims. Similarly Fraisse et al. (2015) estimate that in the 1996-2003 period, "60% of cases end up with a trial, among which 75% lead to a worker's victory".

The decisions of the *Prud'hommes* council are appealed in most of the cases: the appeal rates are, according to Guillonneau and Serverin (2015), between 60% and 67% in the 2004-2013 period. From 2006 to 2016, we find that only 45% of *Prud'hommes* councils decisions about compensations for dismissal were confirmed by Appeal courts. Insofar as appeal rates are very high and the appeal suspends the application of the decisions of *Prud'hommes* councils which are frequently not fully confirmed, the compensation for wrongful dismissals decided at the Appeal court level is a better measure of the compensation to be paid by the firm than that decided by *Prud'hommes* councils.⁷ Therefore, in what follows, we use the compensation for wrongful dismissals decided by Appeal courts.

2.2.2 Overview of Appeal court's organization

There are 36 Appeal courts and 210 *Prud'hommes* councils. Each French Appeal court has different chambers, among which at least one social chamber treats cases coming from the *Prud'hommes* council. Some Appeal courts have several social chambers, such as the Paris court which has fourteen of them. There is one president for each social chamber. This chamber president has administrative responsibilities within the court, and is in charge of presiding over all the chamber's trials. She can nevertheless be replaced whenever needed, for instance during holidays. For each judgment, the chamber president is assisted by two councillor-judges.

The status of judges and their mobility is determined by the *Ordonnance Organique* of 22 December 1958. This regulation states that judges in Appeal courts are 'placed judges', *i.e.* assigned to a given Court or a given Chamber in a specific position according to decisions made every year by the First President of the Court of Cassation (the highest civil jurisdiction) and the First President of the Appeal court. Promotions are based on merit and decided every year by a National Commission of Advancement. The First President of the Appeal court herself is placed by a decree signed by the President of the Republic following the recommendation of the independent National Council of the Judiciary. Besides, mobility requirements are enforced through several regulations, such as promotions awarded only to judges in a given position for less than 5 years in a same jurisdiction (7 years from 2017), the prohibition to

⁷In any case, data about *Prud'hommes* councils decisions are not available.

stay in the same specialized function in the same jurisdiction more than ten years altogether, or geographical mobility requirements to achieve the first grade of the remuneration schedule (organic law 2001-539 of June 25th, 2001). The turnover that follows is substantial: every year 20% of positions are re-assigned among judges (Conseil de la Magistrature, rapport d'activité 2016).

Importantly, the First President of the Appeal court sets objective criteria driving the distribution of the cases between the various chambers of the Appeal court, independently of the judges' identity, under the control of the assembly of judges (articles R312-42 and R312-42-1 of the Judiciary Organisation Code).

2.2.3 Assignment of judges to cases

To identify judge bias, the allocation of cases to judges must be independent of judges observable and non-observable characteristics. Therefore, our identification strategy relies on the quasi-randomness of the allocation of cases to judges. Two aspects of the organization of the judicial system imply that the allocation of judges to cases has important random components, *i.e.* does not depend on the identity of judges.

First, it takes a judge on average two years from the time of her appointment to rule on all the cases assigned to the social chamber *prior* to her arrival. The composition of the court cannot be changed by plaintiffs and judges cannot select their cases, except for conflict of interest. The presence of this backlog and the fact that cases cannot be re-allocated imply that it is almost impossible to assign a case to a specific judge, because the average spell of a judge in a social chamber is equal to about 2.5 years, meaning that the identity of the president that will judge a case assigned to a social chamber is generally unknown when the case is allocated to the social chamber. Moreover, when a president is absent, for vacation, sickness, vocational training or any other reason, she is replaced by the president of another chamber who judges the cases which are scheduled.

Second, the selection of cases settled before going to court can be influenced by the judge in charge of the case. However, employers, workers and lawyers do not know with certainty the identity of the president until the day of the judgment for several reasons: a new judge may be appointed, the judge may be absent and replaced by another one. In addition, in the case of larger Appeal courts, the existence of several social chambers in the same court implies that the social chamber that will judge the case is not known before the judgment.⁸

⁸Our main analysis relies on all Appeal courts, but we show that our results hold when the sample of cases

These institutional features imply that the assignment of judges to cases has important random components that we will leverage to identify the judge bias as explained in Section 2.4.2.

2.3 Data

2.3.1 Compensation data

The empirical analysis draws on a newly created dataset of French Appeal court rulings from 2006 to 2016 bringing together, for the first time, detailed information on compensation amounts decided in court along with a rich set of firm characteristics. From the court rulings, we extract a wide array of variables related to each case, as well as the firm's name and address. Then, using the firm's name and address, we are able to retrieve the firm's unique administrative identifier (*SIREN*), which allows us to link our compensation dataset to comprehensive, matched employer-employee data as well as to financial variables. This section highlights the key steps in the construction of this dataset and the main features of the data. Appendix 2.7.5 provides additional and technical details.

First, we gather 145,638 Appeal court rulings published by the Ministry of Justice. Each of these text documents contains a lot of information in a semi-structured format. Court rulings usually provide a description of the history of the contractual relationship between the employee and the employer. This presentation of facts also includes the claims of the parties and the decision of the *Prud'hommes* council. Court rulings then describe the reasons for the Appeal court decision and end with the compensation for dismissal if the dismissal is deemed wrongful. Figure 2.3.1 shows an extract of a typical ruling.

When her dismissal is ruled wrongful, an employee may receive additional compensations on top of the compensation for wrongful dismissal. Tracking and accounting for these different forms of compensation is important because even though the legal bases for granting them are distinct in principle, judges' full understanding of the case at hand might in practice create correlation patterns between these amounts. In other words, it is possible that a judge's appreciation of the case might color not only the amount granted for unfair dismissal but also the other forms of compensation. Possible additional compensations include: moral and financial damages, compensation for unpaid wages, etc.⁹

is limited to large Appeal courts with several social chambers (see Section 3.5.7).

⁹See Appendix 2.7.5 for a more complete list of the dozens of possible additional compensations.

Figure 2.3.1 – Example of end of Appeal court ruling

PAR CES MOTIFS

LACOUR,

Statuant par arrêt contradictoire,

INFIRME PARTIELLEMENT le jugement déféré et statuant à nouveau,

CONDAMNE la Société [REDACTED] à verser à Monsieur B. 30.000 € (TRENTE MILLE EUROS) à titre d'indemnité pour licenciement sans cause réelle et sérieuse ;

ORDONNE le remboursement par la Société [REDACTED] à l'organisme concerné des indemnités de chômage effectivement versées à Monsieur B. par suite de son licenciement et ce dans la limite de trois mois ;

DÉBOUTE Monsieur B. de sa demande au titre de dommages et intérêts pour manquement aux obligations conventionnelles ;

CONFIRME pour le surplus le jugement déféré ;

Yajoutant,

CONDAMNE la Société [REDACTED] à verser à Monsieur B. la somme de 1.000 € (MILLE EUROS) au titre de l'article 700 du Code de Procédure Civile ;

DÉBOUTE la Société [REDACTED] de sa demande au titre de l'article 700 du Code de Procédure Civile ;

CONDAMNE la Société [REDACTED] aux entiers dépens.

Prononcé publiquement par mise à disposition de l'arrêt au greffe de la Cour, les parties en ayant été préalablement avisées dans les conditions prévues au deuxième alinéa de l'article 450 du Code de Procédure Civile,

Et signé par Madame [REDACTED] président, et par Madame [REDACTED], greffier, auquel la minute de la décision a été remise par le magistrat signataire.

LE GREFFIER LE PRÉSIDENT

Minute en sept pages.

Composition de la juridiction : [REDACTED]
 Décision attaquée : C. Prud. Longwy, Nancy 2011-02-25

Source: Appeal court rulings database.

We extract all these variables automatically from the Appeal court rulings using a Python program based on keywords extraction and natural language processing techniques. In order to control the quality of the process, we assessed the accuracy of the results on a manually-filled dataset forming a subsample of about 2,500 observations, selected at random. We find that the correlation between the compensation amount of the manually-filled and the automatically-filled datasets is equal to 94%, which is in the upper range of seminal papers using this type of approach (Baker, Bloom and Davis (2016)).

Finally, we also retrieve the unique administrative firm identifier known as *SIREN*, either directly from the text when it is displayed, or, using the firm's name and address, after an automatic search on online companies registries such as *societe.com* and *bodacc.fr*. The *SIREN* identifier, assigned by France's statistical agency to each company, then allows us to merge our rulings compensation dataset with French administrative social security and tax data. In some cases where the company is very small or when the cases were launched a long time ago, we were not able to retrieve the *SIREN*.

2.3.2 Social security and tax data

In order to analyze the impact of judge decisions on firm performance, we combine our novel rulings data with two comprehensive administrative datasets. Because both have been used in the literature we only briefly highlight their main characteristics

Matched employer-employee data. We merge the compensation data with social security data thanks to the firm identifier. We use the comprehensive matched employer-employee dataset called DADS Postes *Déclarations Administratives de Données Sociales* from 2002 to 2015, which reports detailed payroll information about each employee working for a French private firm. This dataset allows us to track the evolution over time of the wage bill and of the number of employees of the firms in our rulings dataset.

Tax data. We rely on tax data, FICUS-FARE, that contain the full company accounts, including for instance sales, net income, EBITDA. From these files we are able to construct a wide array of indicators for the firm's financial health such as the firm's leverage ratio, the return on assets, etc. These data are available from 2002 to 2016.

2.3.3 Sample restriction

From our initial sample of 145,638 rulings, we select those for which it is indicated that the firm was not in liquidation at the judgment date, because dismissal compensations of liquidated firms are paid by a public insurance agency (*Agence de Garantie des Salaires*). Since

the parties involved in these cases are no longer the employer and the employee, but the employee and the public agency, these cases are not suitable to identify judge bias in situations where employers are directly involved. Then, we eliminate cases for which the relevant information about the presiding judge's name and surname, the total amount of compensation, and the monthly wage was either not retrieved or is not available. While the most important information is often retrievable – the identity of the Appeal court, compensation amounts for wrongful dismissal, worker's wage and seniority, location of the *Prud'hommes* council, whether the worker or the firm was the appellant, etc. – there are sizeable variations in the amount of available information from one ruling to the next. This heterogeneity reduces the size of the useable sample by about a half. Finally, we eliminate cases in which the employer belongs to the public sector and those judged by judges who have judged less than 50 cases. We end up eventually with 37,149 cases and 159 presidents¹⁰ (See Table 2.3.1). The 159 presidents who judged more than 50 cases cover 93.3% of cases among the universe of cases that we analyze. Each of these presidents judged 450 cases on average with a median equal to 339.

Table 2.3.1 – From the initial to the final number of observations used to estimate judge bias

	# of cases	# of judges
Initial severance pay data	145,638	-
(a) Cases for firms not already liquidated	123,304	-
(b) Cases with non-missing president name and surname	117,989	1,039
(c) Cases with non-missing total amount of compensation	84,151	878
(d) Cases with non-missing monthly wage	61,728	731
(e) Elimination of cases in the public sector	39,843	652
(f) Cases restricted to judges with at least 50 cases	37,149	159

Note: This table presents the selection process to obtain the sample of cases on which we estimate the judge fixed effects. Starting from the initial set of all Appeal court rulings from 2006 to 2016 published by the Ministry of Justice which covers all Appeal court rulings, we apply successive filters in order to retain (a) only those firms that we know were not liquidated at the judgment date, otherwise dismissal compensations of liquidated firms would be incurred by a public insurance agency (*Agence de Garantie des salaires*). Then, we eliminate cases for which we do not have the relevant information about either (b) the president's name and surname, (c) the total amount of compensation, or (d) the monthly wage was either. Finally, we eliminate cases (e) in which the employer belongs to the public sector, and (f) those decided by judges who covered less than 50 cases, our threshold for the calculation of judge fixed-effects. We eventually end up with 37,149 cases and 159 judges. Source: Authors' Appeal court rulings database.

¹⁰Let us remind readers that the court is composed of a president and two councillor-judges. The president, who is in charge of supervising the writing of the judgments, plays the key role in the judgment.

2.4 Judge biases

This section is devoted to the analysis of judge bias. We start by reporting descriptive statistics about judgments before presenting the empirical strategy used to identify judge bias and showing the results.

2.4.1 Descriptive statistics

Table 2.4.1 presents descriptive statistics of judgments at the case level. Our sample comprises only cases that are judged in Appeal courts. The average amount of compensation for wrongful dismissal granted by Appeal courts is equivalent of 4.3 months of salary, while the total amount, including other possible indemnities for unpaid leave, unpaid (overtime) hours worked, unpaid notice, or (more rarely) compensation for damages in case of harassment or discrimination, represents 10.5 months of salary. The worker appeals in 58% of cases.

Table 2.4.1 – Summary main variables of case-level data

	mean	min	med	max	sd	count
Total amount in euro	29,794	0	15,724	963,154	50,056	37,149
Total amount in months of salary	10.47	0	7.84	76.26	11.12	37,149
Positive total amount	0.89	0	1	1	0.31	37,149
Amount for unfair dismissal in euro	12,288	0	3,000	530,000	24,193	37,149
Amount for unfair dismissal in months of salary	4.32	0	1.55	73.17	6.10	37,149
Positive amount for unfair dismissal	0.58	0	1	1	0.49	37,149
Other amount in euro	17,506	0	6,197	963,154	38,024	37,149
Prud'hommes amount	7,326	0	0	277,200	17,649	27,725
Amount demanded by worker	44,458	1	25,000	985,536	64,439	19,371
Higher amount than prud'hommes	0.38	0	0	1	0.49	27,725
Lower amount than prud'hommes	0.17	0	0	1	0.37	27,725
Same amount as prud'hommes	0.45	0	0	1	0.50	27,725
Worker who appealed	0.61	0	1	1	0.49	33,767
Economic dismissal	0.16	0	0	1	0.36	37,149
Worker's seniority in months	81,66	0	50.00	538	87.20	27,147

Note: This table displays the mean, the minimum, the median, the maximum, the standard deviation and the number of observations for several important characteristics of the cases used to estimate judge bias. Source: Appeal court rulings database.

Figure 2.4.1 displays the histogram of the compensation for wrongful dismissal in monthly wages, conditional on being positive. There is a mass around six months of salary: this stems from French legislation that institutes a minimal threshold of six months of salary for workers with more than 24 months of seniority employed in firms with more than 11 workers, when the dismissal is deemed wrongful.

Figure 2.4.1 – Histogram of compensation amounts in monthly wage

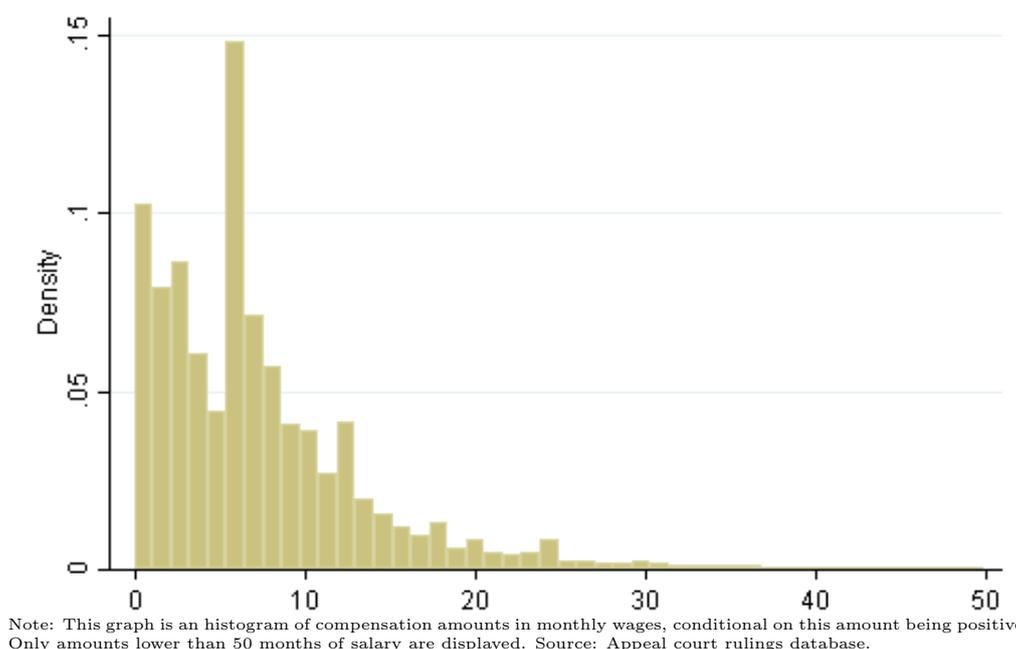


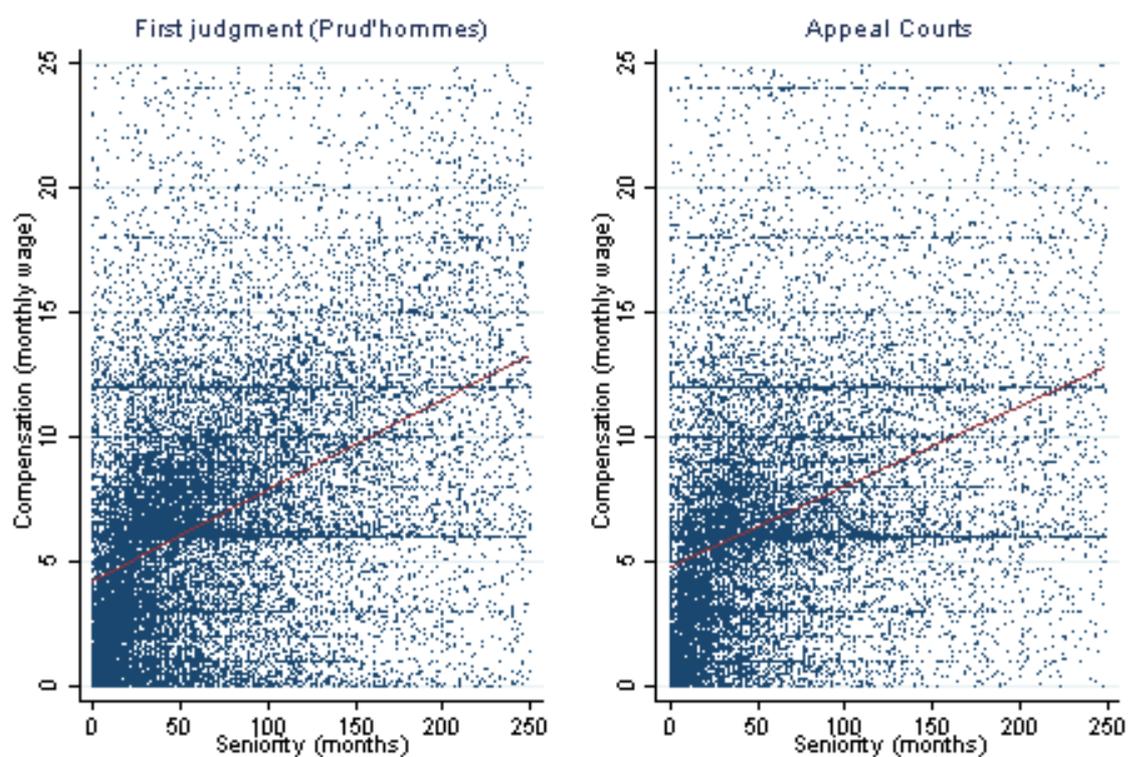
Table 2.4.1 also provides information about differences between decisions of Appeal courts and *Prud'hommes*. The amount given at Appeal court is the same as the amount decided at *Prud'hommes* in 45% of cases, while it is higher in 38% of cases and lower in 17% of cases. The average compensation for unfair dismissal set by Appeal courts is much higher (12.288€) than that of *Prud'hommes* (7.236 €).¹¹ All in all, Appeal courts are more favorable to workers than *Prud'hommes*. Figure 2.4.2 shows the scatter plot of the amount of compensation in monthly wages depending on seniority set by Appeal courts (right panel) and by *Prud'hommes* (left panel). It is apparent that there is an important dispersion of the amount of compensation conditional on seniority in both tribunals. Table 2.4.1 shows that the variance of the compensations of Appeal courts is larger than that of *Prud'hommes*.

Obviously, the variance of compensations conditional on seniority originates from the diversity of situations specific to each case. Nevertheless, the subjective interpretation of judges might exert an important influence, as suggested by the difference between the judgments of *Prud'hommes* and Appeal courts, which is significant at all amounts of compensation (Figure 2.4.3). Only a small share of the variance of compensations is explained by observable case characteristics: for instance, only 13.6% of the variance is explained by salary and seniority. Adding many other covariates¹² makes this share jump to 32.9%. In other words, 67% of the

¹¹Note that we consider here only *Prud'hommes* judgments which are appealed and reach the Appeal court, as the information about other *Prud'hommes* judgments is not available

¹²*i.e.* controlling for the amount granted at *Prud'hommes*, the amount claimed by the worker, the firm's number of workers, whether it was the worker who appealed, whether it is an economic dismissal and the time elapsed between the dismissal and the appeal judgment

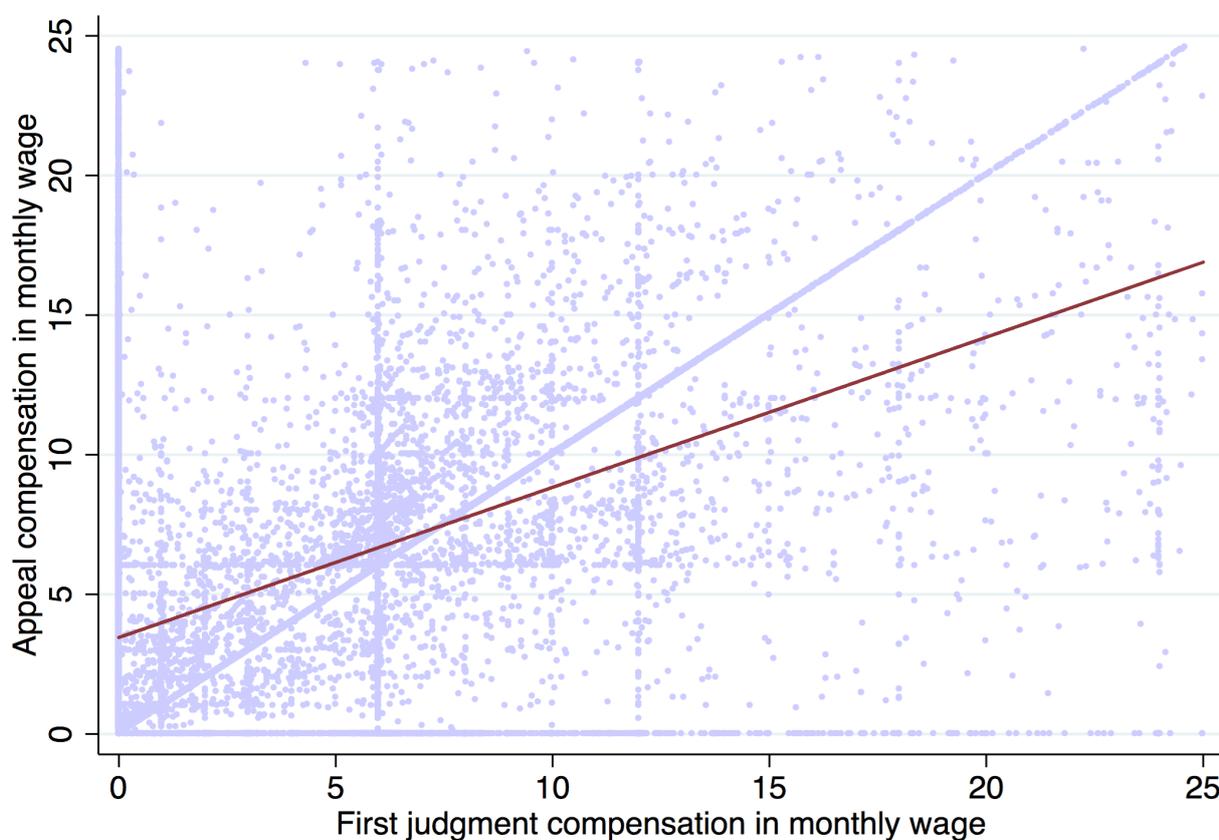
Figure 2.4.2 – Compensations for wrongful dismissals and seniority



Note: top 1% observations trimmed

Note: These graphs are scatter plots of compensations for wrongful dismissals depending on seniority. Compensations are expressed in monthly wage. The left panel displays compensations set by *prud'hommes* and the right panel displays compensations set by Appeal courts. Source: Appeal court rulings database.

Figure 2.4.3 – Relation between compensations for wrongful dismissals set by Appeal courts and by *prud'hommes*



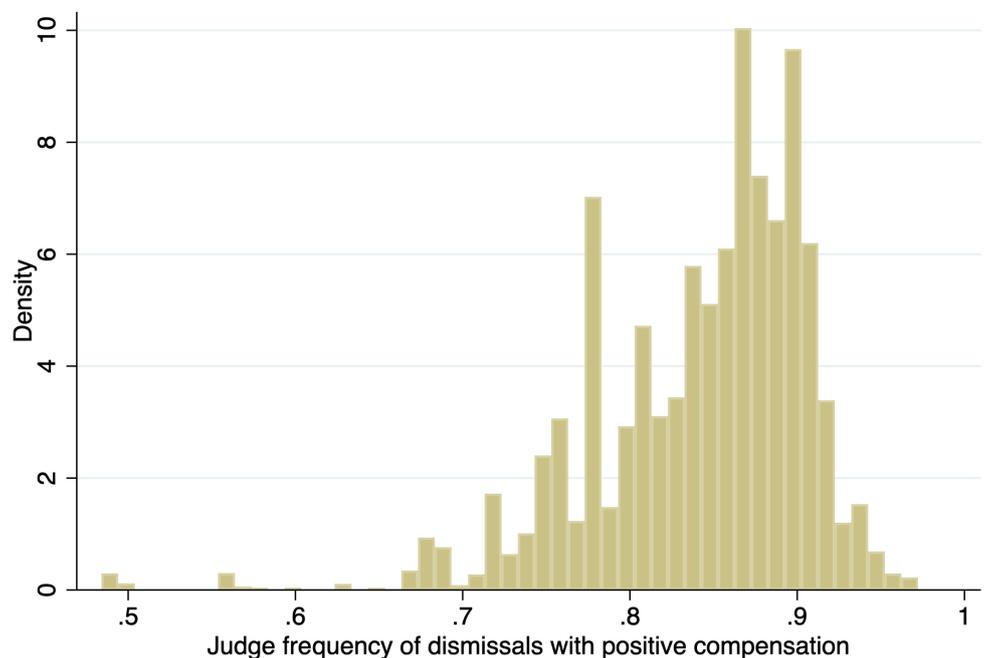
Note: This graph is a scatter plot of the compensations for wrongful dismissals set by Appeal courts and by *prud'hommes*. Compensations are expressed in monthly wage. Source: Appeal court rulings database.

variance of dismissal compensation is still left unexplained when controlling for a wide range of covariates.

We use two types of variable to evaluate a judge's bias: i) the frequency at which the judge grants a positive compensation to the worker (for unfair dismissal or any other motive), and ii) the amount of compensation.¹³ First, Figure 2.4.4 displays the histogram of the frequency at which the judge grants a positive compensation. Figure 2.4.5 shows that amounts granted for unfair dismissal are positively correlated with the amounts granted under other motives. On average one month of salary granted for unfair dismissal is associated with one third of additional monthly wage granted for other motives. In other words, judges' decisions not only bear on amounts granted for unfair dismissal, but also on other compensations related to contract breach, like unpaid hours of work, compensation for non-respect of the dismissal procedure and other reasons enumerated in Section 2.3.1. Therefore, the main variable of interest we use throughout our analysis is the total compensation for contract breach (for

¹³Our measures of Appeal courts judges bias do not rely on the difference between the outcome of the Appeal court and the outcome of *Prud'hommes* insofar as *Prud'hommes*' decisions are influenced by the potential bias of *Prud'hommes* counselors.

Figure 2.4.4 – Histogram of frequency of dismissals deemed unfair per judge



Note: This Figure exhibits the histogram of frequency of dismissals deemed unfair per judge. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

unfair or any other motive), the histogram of which is exhibited in Figure 2.4.6.

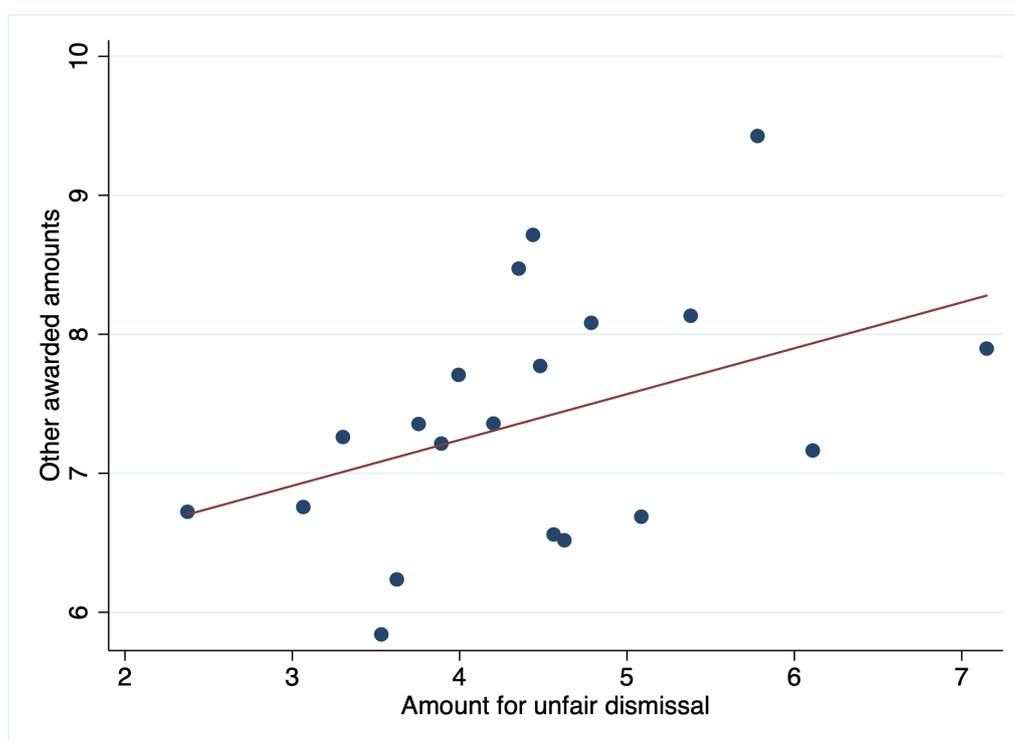
In order to identify the judge bias, the allocation of judges to cases must be random. We devise in the following section our strategy to consistently identify judges biases.

2.4.2 Empirical strategy

Our empirical strategy rests on the assumption that the allocation of judges to cases is random. As argued in Section 2.2.3 this is supported by three key institutional features: i) judges inherit a large backlog, ii) judges are mobile and iii) defendants and plaintiffs have limited information about the identity of the judge which ensures that the personality of judges does not unduly generate case selection through pre-trial settlement. In this context, the random component of the allocation we use is the allocation of cases across different judges within court, social chamber and year. Hence, we rely on differences between decisions of presidents belonging to the *same* social chamber within the *same* year.

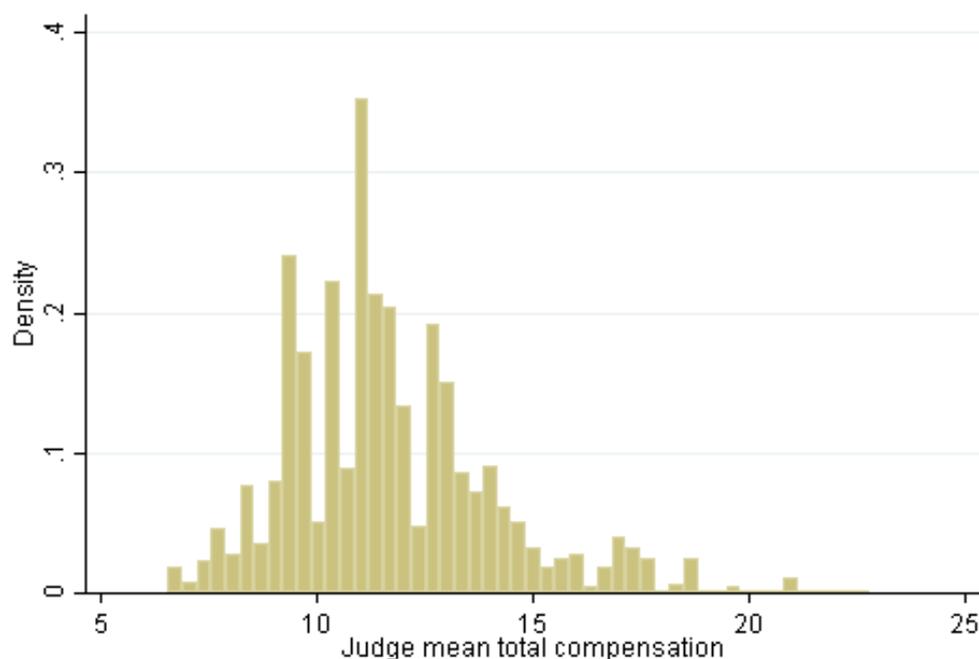
In a given year, the president of a social chamber may move to another job, either to another Appeal court or to another position within the same court, and is then replaced by a new president. The initial judge and the new judge may have different interpretations of labor laws influencing the amount of compensation in case of dismissal. For instance, in year 2014

Figure 2.4.5 – Relation between mean compensation per judge for unfair dismissal and mean compensation granted for other reasons



Note: This figure exhibits the scatter plot of mean compensation in month of salary for unfair dismissal per judge, grouped in 20 equal-sized bins, against the mean compensation for other reasons. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

Figure 2.4.6 – Histogram of mean compensation per judge

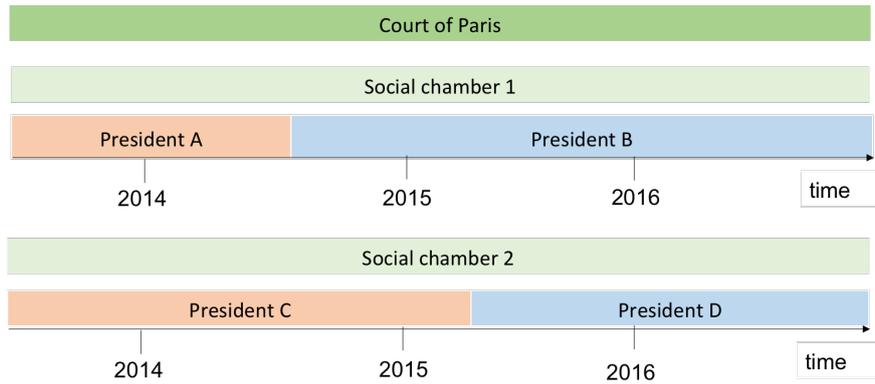


Note: This figure exhibits the histogram of mean compensation in month of salary per judge. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

and social chamber 1 of the Paris Appeal court, a case may be either allocated to president *A* in the first part of the year, or to president *B* in the second part of the year, as shown by Figure 2.4.7. Although unlikely, a non-random assignment of cases to judges is still possible. For instance, it is possible that judge *A* is specialized in sexual harassment cases and that all those cases allocated this year are systematically assigned to this judge. However, what makes such an allocation of cases highly implausible is the large backlog in each social chamber – the average waiting time before judgments is about two years (667 days), and only 10% of cases are judged in less than 300 days. In this context, insofar as the cases are allocated to the social chambers at the start of the appeal procedure, it is very unlikely that cases can be specifically allocated to presidents whose seniority in the chamber is less than one year. Thus, since we rely on differences between decisions of presidents belonging to the same social chamber within the same year to identify judge specific differences, it is unlikely that this identification strategy is burdened by non-random allocation of cases to judges.

Moreover, if the judge is absent the day of the judgment, he can be replaced by another judge without notice to the plaintiff and the defendant. Regardless, the presence of several social chambers implies that the plaintiff and the defendant do not know which social chamber will judge their case before the judgment. This implies that it is very unlikely that the identity

Figure 2.4.7 – Allocation of cases exploited for identification



Note: This figure displays the allocation of cases to judges used for identification. Within an Appeal court, there may be several social chambers. Within each social chamber, there is, at an instant t , one chamber president who judges the cases. When judges change assignments in the course of a year, for instance in 2014, one can identify the allocation to president A or president B.

of the judge in charge of the case influences the settlements before the judgment.

We implement this strategy by computing, for each social chamber \times year pair (k, t) in which we observe judge j , the difference between the average of judge j outcomes¹⁴ in this chamber this year and the average of all outcomes in this chamber this year:

$$\bar{\varepsilon}_{jkt} = \left(\frac{1}{n_{jkt}} \sum_{i \in (j,k,t)} y_i \right) - \left(\frac{1}{n_{kt}} \sum_{i \in (k,t)} y_i \right) \quad (2.1)$$

where $i \in (j, k, t)$ means that case i is judged by judge j in chamber k and year t and $i \in (k, t)$ means that case i is judged in chamber k and year t ; y_i is the outcome of case i ; n_{jkt} the number of judgments of judge j in chamber k during year t and n_{kt} is the number of judgments in chamber k during year t .

Judges move across social chambers during the period. Our measure of the bias of judge j is thus the weighted average of $\bar{\varepsilon}_{jkt}$, where the weight of social chamber k in year t is the share of judgments of judge j in this chamber this year in all judgments of judge j :

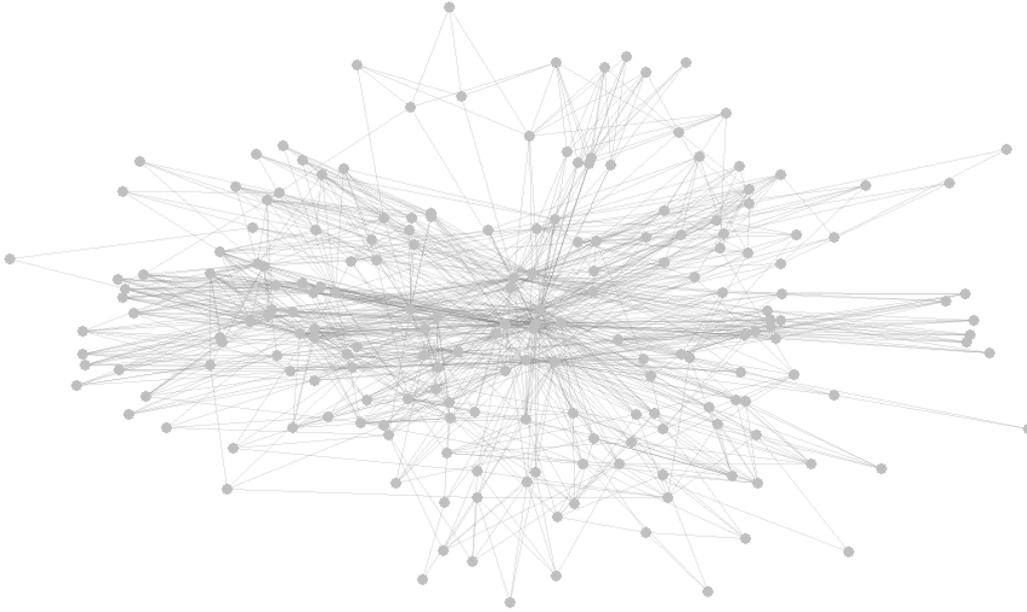
$$\bar{\varepsilon}_j = \sum_{(k,t) \in (K,T)(j)} \frac{n_{jkt}}{n_j} \bar{\varepsilon}_{jkt} \quad (2.2)$$

where $(K, T)(j)$ is the set of all chamber \times year pairs (k, t) observed for judge j ; $\bar{\varepsilon}_j$ is the bias of judge j .

When we analyze the correlation between judge j bias and the outcome of case i , the bias

¹⁴The outcome is either the amount of compensation or the indicator variable equal to one if the dismissal is deemed wrongful.

Figure 2.4.8 – Network of judges



Note: Each dot represents a judge. Two dots are connected if the two judges shared the same social chamber at least once. The higher the network density, the higher the mobility of judges across social chambers. If judges were not mobile whatsoever, one would observe perfectly distinct judge clusters, each cluster representing one social chamber. Source: Appeal court rulings database.

of judge j is measured by the leave-one-out mean of case i , meaning that it is judge specific and case specific. To put it differently, the bias of judge j for case i is¹⁵

$$\bar{\varepsilon}_{ij} = \sum_{(k,t) \in (K,T)(j)} \sum_{i', i' \neq i} \frac{n_{jkt}}{n_j - 1} \bar{\varepsilon}_{i'jkt} \quad (2.3)$$

where

$$\bar{\varepsilon}_{i'jkt} = \left(\frac{1}{n_{jkt} - 1} \sum_{i' \in (j,k,t), i' \neq i} y_{i'} \right) - \left(\frac{1}{n_{kt} - 1} \sum_{i' \in (k,t), i' \neq i} y_{i'} \right) \quad (2.4)$$

Obviously, by definition: $\sum_{i \in j} \bar{\varepsilon}_{ij} = \bar{\varepsilon}_j$.

It is clear that our measure of judge bias relies on their mobility across social chambers which is crucial for comparing all judges. This measure allows us to rank judges according to their bias. The higher the degree of judge mobility, the higher the probability to achieve a perfect ranking (see Appendix 2.7.3). We document the extent of judge mobility in Figure 2.4.8, where each dot represents a judge, and where a line connects two dots if the two judges shared the same social chamber at least once. As is apparent, the network of judges is dense, thus indicating a high mobility of judges across social chambers.¹⁶

¹⁵Note that our definition of the bias can be obtained by regressing the outcome for all cases on chamber \times year fixed effects as in the contributions of Dahl et al. (2014) and Dobbie et al. (2018). See appendix 2.7.2.

¹⁶If judges were not mobile whatsoever, one would observe perfectly distinct judge clusters, each cluster representing one social chamber.

To further confirm the randomness of the allocation of cases to judges, we conduct randomization tests in which we regress our measure of judge specific differences on worker and firm characteristics of corresponding cases. The absence of correlation between observable characteristics of case and judge specific differences indicates that there is no selection on observable variables. Though we obviously cannot test the correlation between judge specific differences and unobserved variables, such randomization tests are reassuring for our identification strategy.

2.4.3 Results

Judge subjectivity can influence both the qualification of the dismissal - either wrongful or lawful - and the compensation amount granted by the judge to the worker. In what follows, we examine these two aspects of judges' decisions and we look at how they are related.

Qualification of dismissals

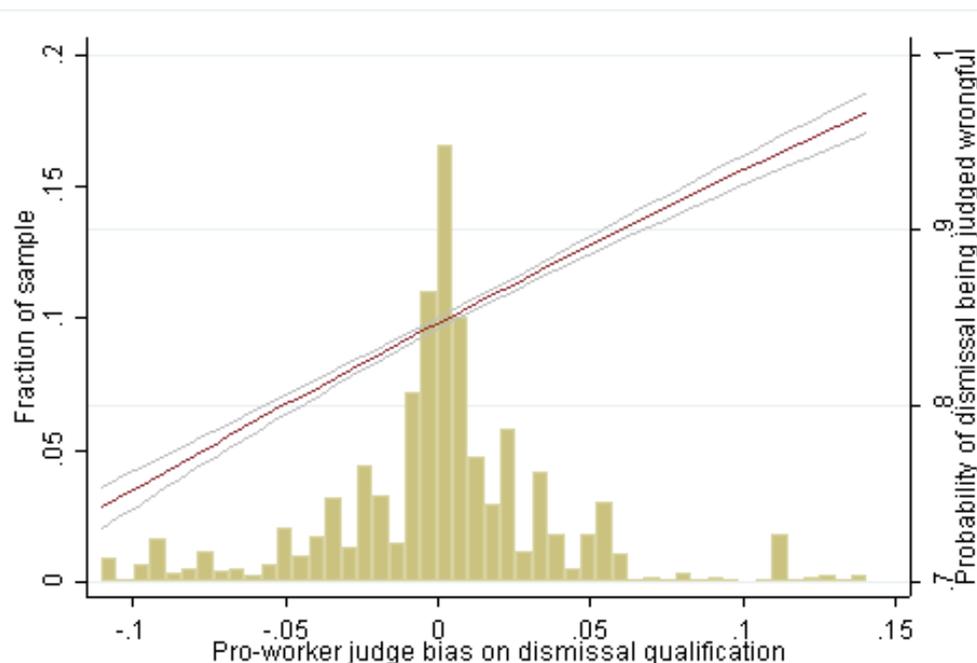
We first construct a judge specific pro-worker bias with respect to the dismissal qualification. Figure 2.4.9 presents the histogram of the judges' pro-worker bias among the population of cases defined by equation (2.3). It sheds light on the variability of biases.

Relation between judge bias and the qualification of dismissals

Our measure of judge bias is relevant only if it is significantly correlated with the qualification of dismissal in each specific case. To check whether our measure of judge bias is indeed related to the actual qualification of dismissals, Figure 2.4.9 displays the local polynomial fit of the probability that dismissals are deemed wrongful explained by the judge pro-worker bias. The judge pro-worker bias is indeed positively related to the probability that dismissals are deemed wrongful. Being assigned to one of the 10% most pro-worker judges as compared to one of the 10% least pro-worker judges increases the probability that the dismissal is deemed wrongful by about 4 percentage points, which corresponds to an increase of 7% in the probability that the dismissal is deemed wrongful.

Table 2.4.2 further documents the relation between the qualification of dismissals and judge pro-worker bias. This table displays the OLS estimator of the regression of the indicator variable equal to one if the dismissal is deemed wrongful on the judge's pro-worker bias. All standard errors are clustered at the judge level. Column (1) includes Appeal court and

Figure 2.4.9 – Judge pro-worker bias with respect to the dismissal qualification



Note: This figure displays the histogram of pro-worker biases of judges with respect to the qualification of dismissals in background and a local polynomial fit of the indicator variable equal to one if the dismissal is deemed wrongful, represented by the red line. The grey lines display the frontiers of the 95% confidence interval of the local polynomial fit. Case-level data are used, therefore the number of observations is the number of different cases for which we are able to compute the pro-worker bias reported in Table 2.4.1. The pro-worker bias is computed as defined in Section 2.4.2. Source: Appeal court rulings database.

year fixed effects. Column (2) adds control variables comprising the worker's salary, seniority and whether the dismissal is economic or for personal reasons. The coefficients, which are significant at 1% level of confidence, are consistent with those obtained from the polynomial fit without any control, displayed on Figure 2.4.9. Indeed, according to Table 2.4.2, being assigned to one of the 10% most pro-worker judges as compared to one of the 10% least pro-worker judges increases the probability that the dismissal is deemed wrongful by 4.9 percentage points¹⁷ which is very close to the prediction of the polynomial fit.

Contribution of judge biases to the dispersion of qualification of dismissals

Table 2.4.3 shows nevertheless that the dispersion of judge fixed effects only explains a small share of the variance of the qualification of dismissal: column (4) exhibits that the adjusted R^2 only increases from 2.7% to 3.0% when controlling for judge bias, once case controls, court fixed effects and year fixed effects are accounted for. One may note that the qualification of the dismissal is barely predicted by fixed effects, case controls and judge bias, indicating

¹⁷The computation is performed as follows: we multiply the point estimate given in column (3) of Table 2.4.2 by the difference of pro-worker bias when going from the 1st to the 9th decile of the pro-worker bias, respectively equal to -0.46 and 0.36.

Table 2.4.2 – Correlation between judge bias and dismissal qualification

	Dismissal qualification (1)	Dismissal qualification (2)
Judge pro-worker bias wrt dismissal qualification	0.508 *** (0.141)	0.493*** (0.133)
Year FE	Yes	Yes
Court FE	Yes	Yes
Case controls	No	Yes
F test	12.91	13.82
# obs	9,138	9,138

Note: Each column corresponds to one regression. The dependent variable is an indicator variable equal to one if the dismissal is deemed wrongful. Court and year fixed effects are included. Control variables included in column (2): indicator variable for economic dismissal, worker's wage, worker's seniority. The top fifth percentiles of judge pro-worker bias are trimmed to account for the non-linearity of the relation between the pro-worker bias and the qualification of dismissal displayed on Figure 2.4.9. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database.

Table 2.4.3 – Share of the variance of compensations explained by judge bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Qualification of dismissal				Compensation in months of salary			
Pro-worker bias	No	Yes	No	Yes	No	Yes	No	Yes
Case controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.020	0.024	0.033	0.037	0.019	0.022	0.114	0.117
Adj. R^2	0.014	0.018	0.027	0.030	0.013	0.016	0.108	0.111
# obs	9,138	9,138	9,138	9,138	9,138	9,138	9,138	9,138

Note: Columns (1) to (4) report the R^2 and adjusted R^2 of the regression of the qualification of the dismissal - i.e dummy indicating whether the dismissal was deemed wrongful - on judge bias and case controls (dummy indicating whether the firm has more than 11 workers at the time of the dismissal, *Prud'hommes* compensation, salary, seniority), while columns (5) to (8) display similar results for the regression of the compensation in monthly salaries. Columns (1) and (5) display the R^2 when adding fixed effects only, columns (2) and (6) when controlling for the judge's pro-worker bias, columns (3) and (7) when controlling case characteristics, column (4) and (8) when controlling for both case characteristics and judge bias. Court and year fixed effects are included in all regressions. Source: Appeal court rulings database.

that a large share of the variation of the qualification is left unexplained when these variables are taken into account.

Analysis of the allocation of cases to judges

If judges are randomly assigned, the addition of control variables in the regression of the qualification of dismissal reported in Column (1) of Table 2.4.2 should not significantly change the estimates of the coefficient of the judge bias, as case characteristics should be uncorrelated with judge bias. The assumption that judges are randomly assigned is not rejected insofar as the coefficients are not significantly different (p-value = 0.25) across specifications reported in

Table 2.4.4 – Randomization test for judge bias with respect to dismissal qualification: case-level characteristics

	Dismissal deemed wrongful	Judge's pro-worker bias
Amount at Prud'hommes (in months)	4.594*** (0.537)	0.0983 (0.106)
Legislation threshold applied	-0.022*** (0.007)	0.001 (0.001)
Seniority	-0.0125 (0.039)	0.0006 (0.005)
Number of employees	-0.001* (0.001)	-0.000 (0.000)
Worker's salary	0.000 (0.000)	0.000* (0.000)
Economic dismissal	0.061*** (0.008)	-0.001 (0.001)
Time between dismissal and Appeal Court	-0.005 (0.007)	-0.002 (0.001)
Joint F-Test	0.0000	0.2291
Observations	9,128	9,128

Note: The dependent variable is an indicator variable equal to one if the dismissal is deemed wrongful in Column (1) and the judge pro-worker bias in Column (2). Court and year fixed effects are included. Standard errors are clustered at the judge level. Standard errors are displayed in parentheses. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database. All independent variables except for 'Legislation threshold applies' and 'Economic dismissal' are transformed to increase clarity of the table: variables are divided by 1000.

Columns (1) and (2) of Table 2.4.2.

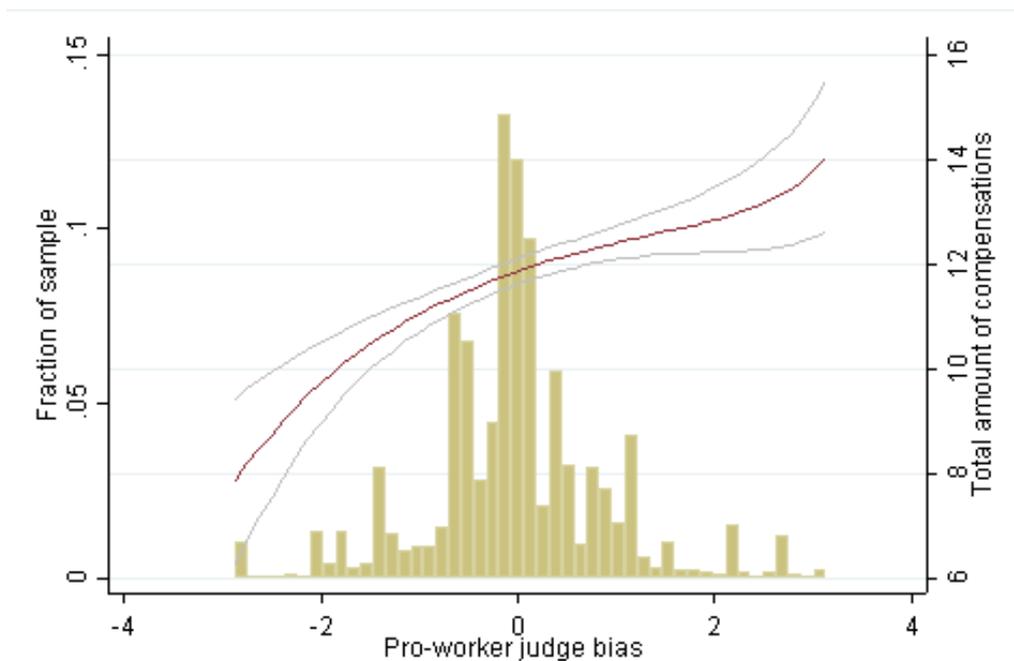
To further check that the measure of judge bias is not the consequence of a non-random allocation of judges to cases, we examine whether judge fixed effects are correlated to the observable characteristics of cases. Tables 2.4.4 and 2.4.5 display such tests. The main finding is that no variable is correlated to judge bias. Table 2.4.4, first column displays the regression of the qualification of the dismissal on several characteristics of the case, with Appeal court and year fixed effects and standard errors clustered at the judge level. The amount granted by *Prud'hommes* and an economic ground for the dismissal are positively correlated with the probability of the dismissal to be deemed wrongful, while the seniority and the fact that the worker appealed are negatively correlated. The second column of Table 2.4.4 thus offers a stark contrast to its first column: when regressing the judge fixed effect on the same characteristics, one finds no significant relationship. Furthermore, the F-test rejects the hypothesis of joint significance of explanatory variables. We replicate the exact same methodology for the characteristics of the firm. Results reported in Table 2.4.5 show that judge bias is not correlated to characteristics of firms.

Table 2.4.5 – Randomization test for judge bias with respect to dismissal qualification: firm-level characteristics

	Dismissal deemed wrongful	Judge's pro-worker bias
Number of workers in t-1	-0.153 (0.131)	0.032 (0.023)
Sales in t-1	0.000 (0.001)	-0.000 (0.000)
Total wages in t-1	-0.009 (0.010)	-0.002 (0.002)
Value added in t-1	0.007 (0.007)	0.001 (0.001)
Net income in t-1	0.011 (0.012)	0.000 (0.002)
Debt in t-1	0.002 (0.003)	0.000 (0.000)
Cash in t-1	-0.014** (0.007)	-0.000 (0.001)
Joint F-Test	0.2313	0.8956
Observations	4,847	4,847

Note: The dependent variable is an indicator variable equal to one if the dismissal is deemed wrongful in Column (1) and judge pro-worker bias in Column (2). Court and year fixed effects are included. Standard errors clustered at the judge level. Standard errors are displayed in parentheses. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database. All independent variables are transformed to increase clarity of the table: variables are divided by 1000.

Figure 2.4.10 – Judges pro-worker biases with respect to the compensation in months of salary



Note : This figure displays the histogram of the pro-worker biases of judges with respect to the total amount of compensation for wrongful dismissal and a local polynomial fit of the total amount of compensation, represented by the red line. The grey lines display the frontiers of the 95% confidence interval of the local polynomial fit. Case-level data are used, therefore the number of observations is the number of different cases for which we are able to compute the pro-worker bias reported in Table 2.4.1. The pro-worker bias is computed as defined in Section 2.4.2. Source: Appeal court rulings database.

Compensation for wrongful dismissal

The amount of the full compensation package granted by the judge provides another dimension along which to analyze judges' heterogeneity. In the following, we perform the same exercise as before by computing the pro-worker bias based on the amount granted by the judge as a proportion of monthly wage. Figure 2.4.10 presents the histogram of the judge bias among the population of cases. The judge bias displays a significant heterogeneity.

Relation between the judge bias and the amount of compensation for wrongful dismissal

The judge bias computed from the amount of compensation is highly correlated to the compensation granted by the judges. This correlation is illustrated by Figure 2.4.10 which displays the polynomial fit of the compensation explained by judge pro-worker bias. Being assigned to one of the 10% most pro-worker judges rather than one of the 10% least pro-worker judges increases the amount by about 2 months of salary.¹⁸

Table 2.4.6 provides further evidence about the relation between the compensation granted by the judges and their bias computed with the amount of compensation. Table 2.4.6 displays

¹⁸The judge bias of the 1st decile is equal to -1.28 and that of the 9th decile to 1.25.

Table 2.4.6 – Correlation between judge bias and compensation for wrongful dismissal

	Compensation (1)	Compensation (2)
Judge pro-worker bias wrt compensation	0.852 *** (0.241)	0.838*** (0.241)
Year FE	Yes	Yes
Court FE	Yes	Yes
Case controls	No	Yes
F test	12.47	12.10
# obs	9,138	9,138

Note: Each cell corresponds to one regression where the dependent variable is the total compensation for wrongful dismissal. Control variables included in column (2): indicator variable for economic dismissal, wage, seniority. The bottom and top fifth percentiles of judge bias are trimmed to account for the non-linearity of the relation between judge bias and the qualification of dismissal displayed on Figure 2.4.10. Court and year \times sector fixed effects are used. Standard errors, clustered at the judge level, are in parenthesis. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database.

the OLS estimators of the regression of the compensation for wrongful dismissal in monthly wages on the judge's pro-worker bias. Column (1) reports the result with Appeal court and sector \times year fixed effects. Column (2) adds control variables comprising the worker's salary, seniority and whether the dismissal is economic or for personal reasons. Controlling for case characteristics, an increase in the judge pro-worker bias by one point increases the amount of compensation in months of salary by 0.8 points. This implies that being assigned to one of the 10% most pro-worker biased judges as compared to one of the least 10% pro-worker judges increases the compensation amount by 2.1 months of salary. This prediction is in line with that obtained from the polynomial fit, displayed on Figure 2.4.10.

Contribution of judge biases to the dispersion of compensation for wrongful dismissal

Although judge biases are strongly correlated with the amount of compensation, Table 2.4.3 shows that the dispersion of judge bias only explains a small share of the variance of compensations for wrongful dismissals: Column (8) exhibits that the adjusted R^2 only increases from 10.8% to 11.1% when controlling for the judge bias once case controls, court fixed effects and year fixed effects are accounted for. This suggests that the judge bias may explain a limited share of the large dispersion of compensation conditional on several observable characteristics of cases.

Analysis of the allocation of cases to judges

As before, if judges are randomly assigned, the addition of control variables in the re-

gression of the amount of compensation reported in Column (1) of Table 2.4.6 should not significantly change the estimates of the coefficient of the judge bias, as cases characteristics should be uncorrelated with judge bias. The assumption that judges are randomly assigned is not rejected insofar as the coefficients are not significantly different (p-value = 0.71) across specifications reported in Columns (1) and (2) of Table 2.4.6.

Furthermore, judge biases are not correlated with the observable characteristics of cases or firms. Tables 2.4.7 and 2.4.8 display respectively the correlation between pro-worker biases and the characteristics of the case, and the correlations between pro-worker biases and the characteristics of the firm. The amount received at *Prud'hommes*, the seniority of the worker, and the worker's salary are all positively correlated to the compensation granted at Appeal court. The second column of Table 2.4.7 therefore offers a sharp contrast to its first column: when regressing the pro-worker bias on the same characteristics, one finds no significant relationship. The second column of Table 2.4.8 displays the regression of the judge's severity on the firm's characteristics the year before the judgment, ie in $t-1$. No significant relationship is found.

Judges who often qualify the dismissal as wrongful are also those who, conditional on granting a positive compensation, grant the highest compensations. In other words, our two indices of pro-worker bias are highly and positively correlated. We display this correlation in Figure 2.4.11, which presents the scatter plot of the pro-worker bias with respect to the compensation granted, conditional on being positive,¹⁹ and the pro-worker bias with respect to the dismissal qualification.

All in all, our analysis of Appeal court rulings points to the existence of significant biases on the part of judges which influence the probability that dismissals are deemed wrongful and the amount of compensation for wrongful dismissal. However, the dispersion of judge biases only explains a very limited share of the dispersion of the qualification of dismissal and of the amount of compensation, conditional on observable characteristics of the cases, suggesting that judges interpret in a similar way many specific features of cases which are not observable without very detailed information about each specific case. The next section analyzes the consequence of judge bias on firms performance.

¹⁹Note that Figure 2.4.10 reports judges biases for the average compensation unconditional on being positive.

Table 2.4.7 – Randomization test for judge bias on total compensation for wrongful dismissal: case-level characteristics

	Compensation in monthly wages	Judge pro-worker bias in monthly wages
Amount at Prud'hommes (in months)	0.536*** (0.083)	-0.002 (0.002)
Legislation threshold applied	0.116 (0.386)	0.013 (0.027)
Seniority	0.019*** (0.004)	0.000 (0.000)
Number of employees	-0.000 (0.000)	-0.000 (0.000)
Worker's salary	-0.000*** (0.000)	0.000 (0.000)
Economic dismissal	1.116 (0.683)	-0.025 (0.030)
Time between dismissal and Appeal Court	0.001 (0.001)	-0.000 (0.000)
Joint F-Test	0.0000	0.7458
Observations	4,948	4,948

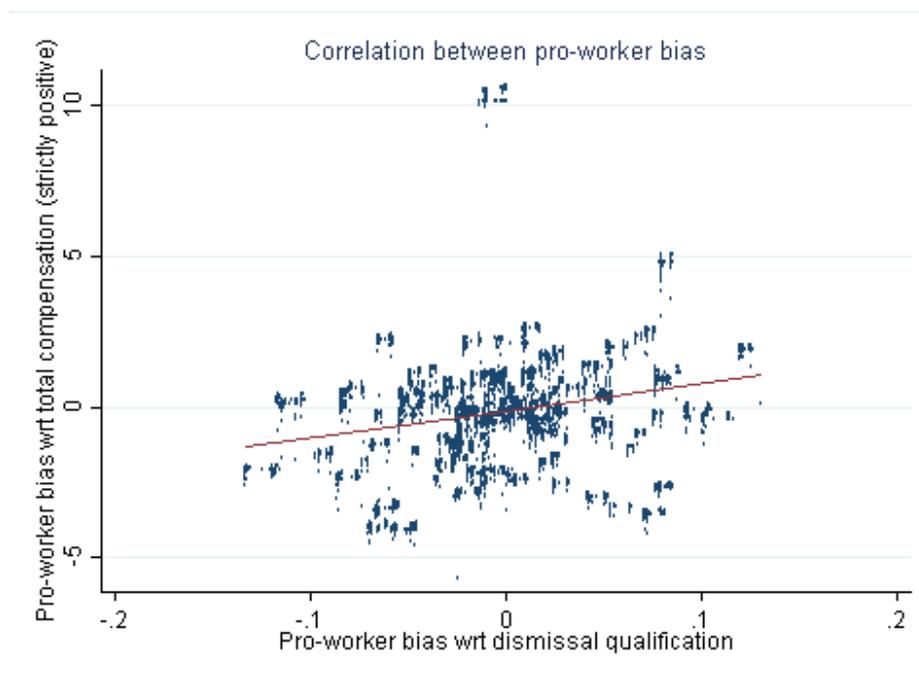
Note: The dependent variable in the first column is the total compensation for wrongful dismissal. The dependent variable in the second column is the judge pro-worker bias computed as defined in section 2.4.2. Standard errors are displayed in parentheses. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales, and an indicator variable for economic dismissals. Standard errors clustered at the judge level. Standard errors, clustered at the judge level, are in parenthesis. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database.

Table 2.4.8 – Randomization test for judge bias on compensation for wrongful dismissal: firm-level characteristics

	Compensation in monthly wages	Judge pro-worker bias in monthly wages
Number of workers in t-1	11.120* (5.763)	0.778 (0.537)
Sales in t-1	0.042 (0.033)	-0.003* (0.002)
Total wages in t-1	-0.586 (0.497)	-0.066 (0.043)
Value added in t-1	0.315 (0.277)	0.024 (0.022)
Net income in t-1	-0.713 (0.676)	0.009 (0.043)
Debt in t-1	0.055 (0.140)	0.011 (0.010)
Cash in t-1	-0.161 (0.201)	-0.017 (0.013)
Joint F-Test	0.1312	0.2241
Observations	4,847	4,847

Note: The dependent variable in the first column is an indicator variable equal to one if the dismissal is deemed wrongful. The dependent variable in the second column is the judge pro-worker bias computed as defined in section 2.4.2. Standard errors are displayed in parentheses. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales, and an indicator variable for economic dismissals. All independent variables are transformed to increase clarity of the table: variables are divided by 1000. Standard errors are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 2.4.11 – Correlation between the two indices of pro-worker biases



Note: This figure is a scatter plot of the pro-worker bias measure computed from the dismissal qualification and the pro-worker bias computed from the compensation amount, conditional on being positive. Pro-worker biases are computed as defined in Section 2.4.2. Source: Appeal court rulings database.

2.5 The effects of judge bias on firm performance and firm survival

This section is devoted to the analysis of the impact of judge bias on firms' performance. We start by presenting some descriptive statistics on firms, then proceed to an event study, before presenting the main empirical strategy and results. Finally, we exploit the results to explore the consequences of reducing the dispersion of judge bias before proceeding to several robustness checks.

2.5.1 Descriptive statistics

The analysis is focused on firms with fewer than 100 employees the year before the Appeal judgment, because the decisions of judges should in principle have stronger effects on small firms. We consider for-profit firms in the private sector, excluding the agricultural sector. Among the sample of appeal court rulings going from 2006 to 2016, we select firms going to court no later than 2012 in order to analyze outcome variables up to three years after the judgment.²⁰ We drop firms going to court several times during the period in order to drop

²⁰Matched employer-employee data are available from 2002 to 2015.

collective dismissals. The description of sample restrictions is presented in Table 2.5.1.

Table 2.5.1 – From the initial to the final number of observations used to estimate the effect of judge bias on firm performance

	# of obs.	# of firms	# of judges
Sample with judge fixed effects	82,320	13,995	159
Non-missing employment, wages, Roa	40,280	5,035	129
Firms with less than 100 employees	35,888	4,486	129

Note: The final sample is restricted to private firms, with less than 100 employees the year preceding the judgement, which go to Appeal courts for individual dismissals. Employment: headcounts on 31 December before the judgment year; Wages: gross monthly wage; Roa: Return on assets. In this table, the number of observations corresponds to the number of cases \times the number of years in the sample. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.2 provides descriptive statistics at the firm level, *i.e.* the level of analysis for our sample. Because we restrict the analysis to firms under 100 employees, the average number of workers is about 20 employees. The firms are relatively young as 24% are less than 10 years old. 52% of firms end up paying a positive compensation for wrongful dismissal. For firms paying a positive compensation amount, which corresponds on average to 10.7% of firms' annual payroll, the median is equal to 4.1%. Their probability to survive one year after the judgment is equal to 99% and to 92% three years after.

Table 2.5.2 – Summary of main variables at firm-level - all firms (< 100 employees)

	mean	min	med	max	sd	count
Nb of workers	20.08	1.00	12	99.00	20.55	4486
Sales (in K euros)	4788.61	0.00	1929.91	64,175 .00	7,429.92	4428
Share of firms in manufacturing	0.17	0.00	0.00	1.00	0.38	4486
Share of firms in construction	0.11	0.00	0.00	1.00	0.31	4486
Share of firms in services	0.34	0.00	0.00	1.00	0.48	4486
Share of firms < 10 years	0.24	0.00	0.00	1.00	0.43	4486
Survival at t+1	0.99	0.00	1.00	1.00	0.11	4486
Survival at t+2	0.96	0.00	1.00	1.00	0.18	4486
Survival at t+3	0.92	0.00	1.00	1.00	0.26	4486
Wrongful dismissal	0.52	0.00	0.00	1.00	0.50	4486
Amount in month of salary (when >0)	11.07	0.01	8.08	197.47	12.25	3009
Amount in payroll (%) (when >0)	10.75	0.00	4.06	149.75	18.56	3805
Judge pro-worker bias	-0.03	-2.76	-0.01	2.22	0.76	4486

Note: "Nb of workers" corresponds to headcounts on 31 December before the judgment year. "Amount" stands for the total amount of compensation. "Wrongful dismissal" is a dummy variable equal to one if the dismissal is deemed wrongful. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

For small firms below 10 employees (see Table 2.5.3), for which the judge bias will be shown to have more impact, the probability of wrongful dismissal is identical but the share of compensation for wrongful dismissal (conditional on being positive) in the annual payroll is

much higher; it is equal to about 20.9% for small firms versus 10.7% for the others. Small firms are younger than larger firms as 35% have less than 10 years versus 25%, and their survival probability is significantly smaller: 89% three years after the judgment versus 92%.

Table 2.5.3 – Summary of main variables at firm-level - small firms (< 10 employees)

	mean	min	med	max	sd	count
Nb of workers	4.98	1.00	5.00	9.00	2.38	1902
Sales (in K euros)	1231.21	0.00	684.44	24,990.51	2136.92	1902
Share of firms in manufacturing	0.11	0.00	0.00	1.00	0.32	1902
Share of firms in construction	0.10	0.00	0.00	1.00	0.30	1902
Share of firms in services	0.35	0.00	0.00	1.00	0.48	1902
Share of firms < 10 years	0.35	0.00	0.00	1.00	0.48	1902
Survival at t+1	0.98	0.00	1.00	1.00	0.15	1902
Survival at t+2	0.95	0.00	1.00	1.00	0.22	1902
Survival at t+3	0.89	0.00	1.00	1.00	0.31	1902
Wrongful dismissal	0.52	0.00	0.00	1.00	0.50	1902
Amount in month of salary (when >0)	9.75	0.01	7.23	78.15	9.78	1299
Amount in annual payroll (%) (when >0)	19.28	0.00	10.56	381.36	24.56	1620
Judge pro-worker bias	0.00	-2.76	-0.01	2.22	0.77	1902

Note: “Nb of workers” corresponds to headcounts on 31 December before the judgment year. “Amount” stands for the total amount of compensation. “Wrongful dismissal” is a dummy variable equal to one if the dismissal is deemed wrongful. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

2.5.2 Empirical strategy

Before presenting our main specification, we start by performing an event study to provide some insights on the impact of judge bias. The event study compares employment growth relative to the year preceding the judgment between two groups of firms: (i) firms which face a pro-worker judge – whose bias is above the median – and (ii) firms which face a pro-employer judge – whose bias is below the median. This approach amounts to implementing a dynamic difference-in-differences design to estimate how judge bias affects employment growth. This approach has two advantages. First, outcome variables can be observed for both groups before the judgment so that the common trend assumption, which should be satisfied if the type of judge does not induce selection of cases going to courts, can be evaluated directly. Second, the research design allows for a transparent graphical assessment of the impact of judge bias over time. Formally, our estimate of the effect of judge bias is based on the following model

$$Y_{ik} = \alpha + \gamma_{c(i)} + \sum_{k=-3}^3 \beta_k \times \mathbf{1}_k + \sum_{k=-3}^3 \beta_k^{\text{proworker}} \times \mathbf{1}_k \times \text{proworker}_i + \epsilon_{ik} \quad (2.5)$$

where Y_{ik} stands for the outcome of firm i in year k relative to the judgment year, proworker_i is an indicator variable equal to one for firms judged by pro-worker judges and to zero otherwise,

$\mathbf{1}_k$ is a year k fixed effect. The model includes social chamber fixed effects $\gamma_{c(i)}$ insofar as we want to compare the outcomes of firms judged in the same social chamber by different judges. ϵ_{ik} is an error term. The baseline specification does not include other covariates to check whether the common trend holds even without conditioning on any observable variable, meaning that no selection due to the type of judge occurs before the judgment.

Our main empirical specification explores in more detail the impact of judge pro-worker bias on an array of firm performance indicators: firm survival, growth of total, temporary and permanent employment and sales. We also analyze non-linearities of these effects, which are important to study the role of the dispersion in bias. The benchmark equation is the following:

$$Y_{ij(i)t} = \alpha_0 + \alpha_1 bias_{ij(i)} + \alpha_2 X_{it} + \eta_{ij(i)t} \quad (2.6)$$

where $Y_{ij(i)t}$ is the outcome of interest for firm i assigned to judge j , $t \geq 0$ years after the judgment; $bias_{ij(i)} = (\bar{\epsilon}_{ij} - \bar{\epsilon}) / \sigma_\epsilon$ denotes judge j 's normalized bias (i.e., the difference between the judge's bias and the average judge bias ($\bar{\epsilon}$) scaled in standard deviation (σ_ϵ) units of the judge bias distribution), where $\bar{\epsilon}_{ij}$, defined in Section 2.4.2, is the leave-one-out mean of the residuals for all the other cases than i judged by the corresponding judge j . X_{it} includes Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. We also proceed to estimations including quadratic terms on the judge bias to account for potential non-linearities.

We estimate Equation (2.6) with OLS and the condition for α_1 to be unbiased is that the error term $\eta_{ij(i)t}$ is mean-independent of $bias_{ij(i)}$. A necessary condition for unbiasedness is random assignment of judges to case. Although this condition is fundamentally non-testable, the random nature of the assignment of judges to cases has been documented above in Sections 2.2.3 and 2.4 and confirmed by the results of the event study in Section 2.5.3 below.

Equation (2.6) allows us to analyze the average impact of the bias of judges on all firms. However, it is probable that firms which do not perform well are more impacted by the high compensations set by pro-worker judges. To deal with this issue, we examine how the impact of judge bias depends on the return on assets. More precisely, we estimate the following equation:

$$Y_{ij(i)t} = \beta_0 + \beta_1 bias_{ij(i)} \times low_i + \beta_2 bias_{ij(i)} \times high_i + \beta_3 X_{it} + \nu_{ij(i)t} \quad (2.7)$$

where low_i is an indicator variable equal to 1 if the financial variable (i.e. the return on assets or the leverage) of firm i the year before the judgment is below the median; $high_i$ is an

indicator variable equal to 1 if the financial variable of firm i the year before the judgment is above the median. X_{it} includes the same variables as before plus the indicator variables low_i and $high_i$.

Our dependent variables include indicator variables equal to one for firms which survive within $t = 1, 2, 3$ years after the judgment and symmetric growth rates for a set of variables, namely total, temporary and permanent employment and sales.²¹ All standard errors are clustered at the judge level, following Abadie, Athey, Imbens and Wooldridge (2017) who state that the standard errors clustering must be decided according to the level at which either the sampling or the randomization is performed. In our case, the randomization occurs primarily at the judge level.

Moreover, in order to quantify the impact of the shock on the amount of compensation induced by judge bias on the performance of firms, we regress the performance indicators on the share of the compensation for wrongful dismissal in the firm payroll, which is instrumented by the judge's bias. This allows us to evaluate the impact of unexpected shocks on the amount of compensation, expressed in payroll share, on firms.

2.5.3 Results

Event study

The event study shows that significant differences in employment growth emerge between firms judged by pro-worker judges and firms judged by pro-employer judges after judgement, in spite of common pre-judgement trends. Figure 2.5.1 displays the average employment growth difference between these two groups of firm before and after the judgment estimated according to equation (2.5). The left top panel reports the results for all firms under 100 employees and the right top panel for firms under 100 employees whose return on assets is below the median. The bottom panel provides similar graphs for firms under 10 employees.

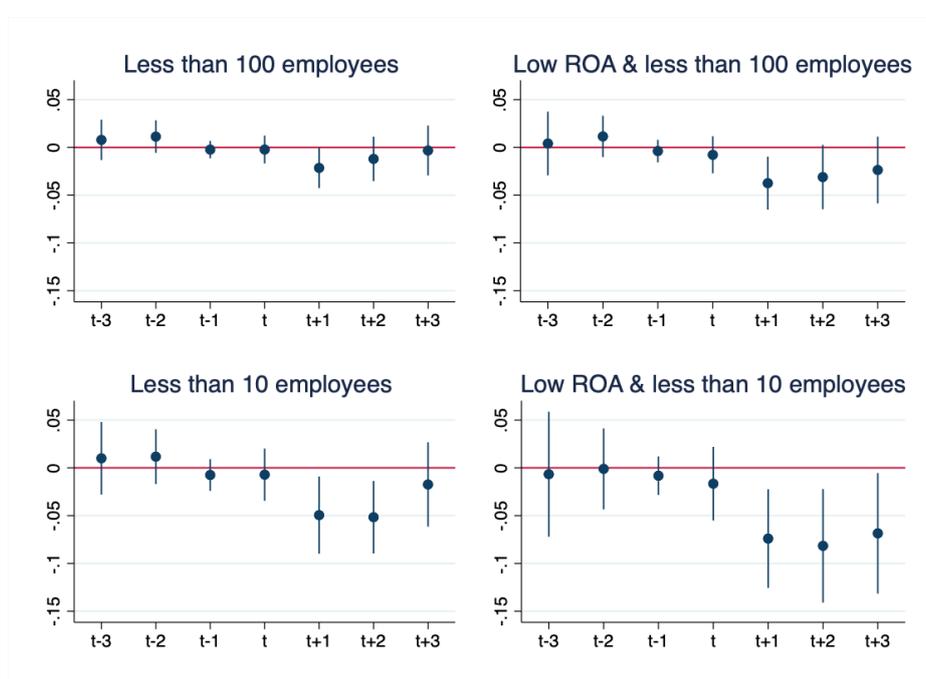
It is clear that there is no significant employment growth difference between the two groups of firm in the three years preceding the judgment date. This confirms the assumption that the type of judge does not influence the selection of firms which go to the judgment, even

²¹For instance, the symmetric growth rate between $t-1$ and $t+1$ is computed as follows:

$$\Delta Y_{ij(i)t} = 2 \frac{Y_{ij(i)t+1} - Y_{ij(i)t-1}}{Y_{ij(i)t+1} + Y_{ij(i)t-1}} \quad (2.8)$$

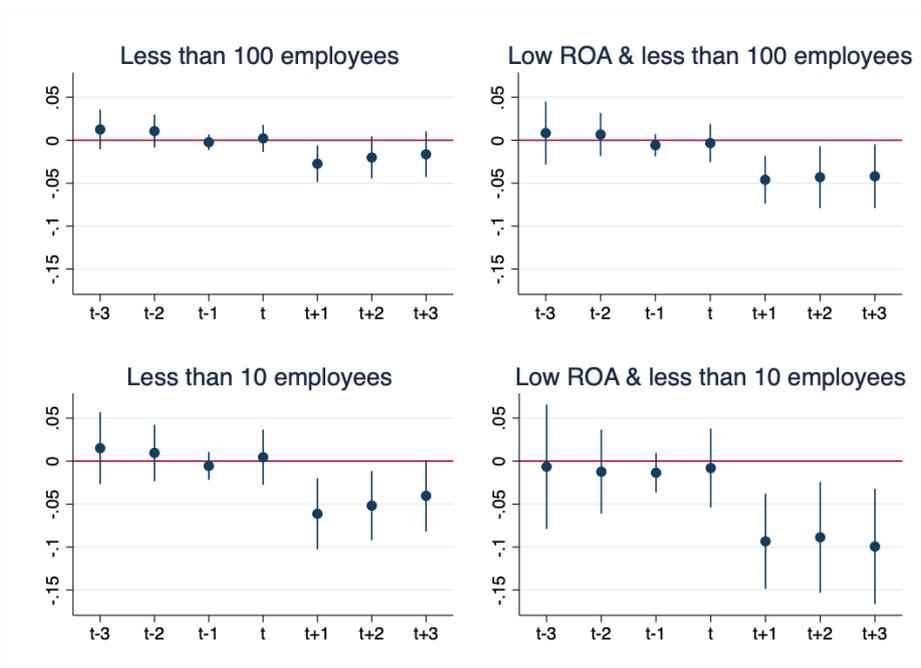
This growth rate measure has become standard in analysis of establishment and firm dynamics because it shares some useful properties of log differences and accommodates entry and exit. It is a second-order approximation of the log difference for growth rates around 0 and it ensures that growth rates range from -2 to 2, thus preventing outliers from complicating the analysis. See Törnqvist, Vartia and Vartia (1985) and Davis, Haltwanger and Schuh (1996)

Figure 2.5.1 – Event study: employment growth rate difference between firms judged by pro-worker and pro-employer judges



Note: This figure displays the average difference in symmetric employment growth rates relative to the year preceding the judgment year t between the group of firms which face a pro-worker judge, whose bias is above the median, and the group of firms which face a pro-employer judge, whose bias is below the median. The average difference in year $k, k \in [-3, 3]$ relative to the judgment year t is equal to coefficient $\beta_k^{proworker}$ of equation (2.5). The left top panel reports the results for all firms under 100 employees and the right top panel for firms under 100 employees whose return on assets is below the median. The bottom panel provides similar graphs for firms with less than 10 employees. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 2.5.2 – Event study: employment growth rate difference between firms judged by pro-worker and pro-employer judges (with control variables)



Note: This figure displays the average difference in symmetric employment growth rates relative to the year preceding the judgment year t between the group of firms which face a pro-worker judge, whose bias is above the median, and the group of firms which face a pro-employer judge, whose bias is below the median. The average difference in year $k, k \in [-3, 3]$ relative to the judgment year t is equal to coefficient $\beta_k^{proworker}$ of equation (2.5) controlling for year fixed effect, firm age, an indicator variable for economic dismissals, the return on assets in the previous year and the leave-one-out average industry annual growth rate of sales. The left top panel reports the results for all firms under 100 employees and the right top panel for firms under 100 employees whose return on assets is below the median. The bottom panel provides similar graphs for firms with less than 10 employees. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

on observable characteristics, since equation (2.5) is estimated without other control variables than the social chamber fixed effects.

The year after the judgment, a difference in employment growth begins to arise at the expense of firms facing pro-worker judges. This difference is small and short-lived in firms below 100 employees. But the difference is larger and more long-lasting in less profitable and smaller firms with less than 10 employees. Three years after the judgment, the difference amounts to 7 percentage points in small, low-profitable firms. Figure 2.5.2 shows that these results hold when employment growth differences are conditional on a set of covariates including year fixed effects, firm age, an indicator variable for economic dismissals, the return on assets in the previous year and the leave-one-out average industry annual growth rate of sales. The absence of statistical significant difference between the results obtained with and without control variable confirms once again the absence of selection of cases going to judgement according to the type of judge.

Figures 2.5.3 and 2.5.4 show that the employment impact of judge bias stems from per-

Figure 2.5.3 – Event study: permanent employment growth rate difference between firms judged by pro-worker and pro-employer judges



Note: This figure displays the average difference in symmetric permanent employment growth rates relative to the year preceding the judgment year t between the group of firms which face a pro-worker judge, whose bias is above the median, and the group of firms which face a pro-employer judge, whose bias is below the median. The average difference in year $k, k \in [-3, 3]$ relative to the judgment year t is equal to coefficient $\beta_k^{proworker}$ of equation (2.5). The left top panel reports the results for all firms under 100 employees and the right top panel for firms under 100 employees whose return on assets is below the median. The bottom panel provides similar graphs for firms with less than 10 employees. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

manent jobs only: the growth rate of temporary employment (i.e. fixed term contracts) does not diverge between the two groups of firms after the judgment date while that of permanent jobs diverges significantly.

Reduced form estimates

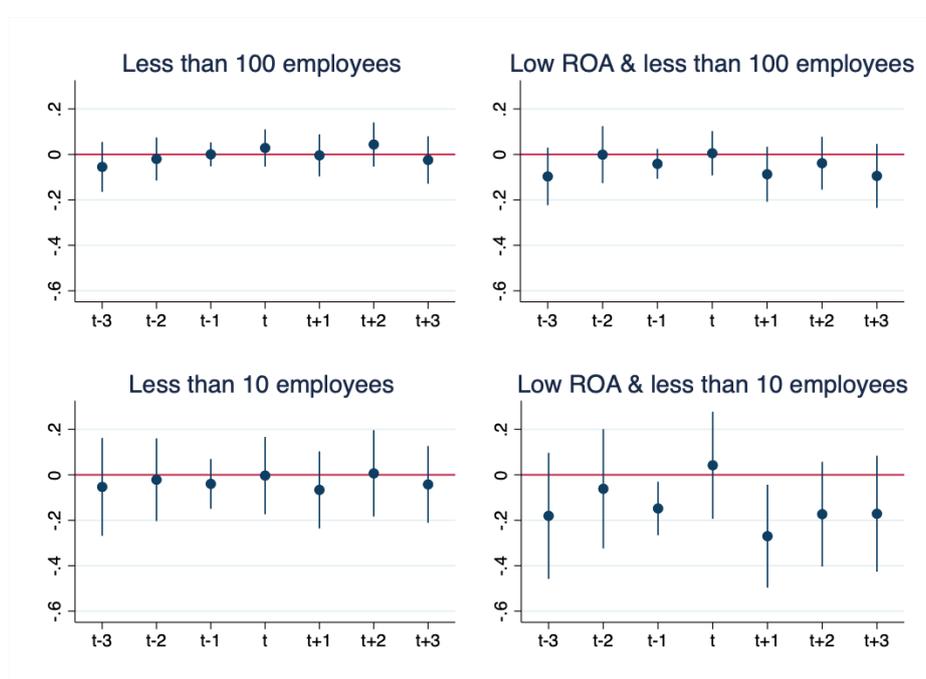
We start by presenting the results of the effects of judge bias on all firms below 100 employees before looking at the impact of judge bias according to firm size, and especially small firms, below 10 employees.

All firms below 100 employees

Tables 2.5.4, 2.5.5 and 2.5.6 present the results of the estimation of equations (2.6) and (2.7) for the firm's outcomes respectively 1 year, 2 years and 3 years after the Appeal court judgment.

Table 2.5.4 shows that the pro-worker bias of judges has a significant negative impact on employment growth the first year after the judgment only for firms with low return on assets.

Figure 2.5.4 – Event study: temporary employment growth rate difference between firms judged by pro-worker and pro-employer judges



Note: This figure displays the average difference in symmetric temporary employment growth rates relative to the year preceding the judgment year t between the group of firms which face a pro-worker judge, whose bias is above the median, and the group of firms which face a pro-employer judge, whose bias is below the median. The average difference in year $k, k \in [-3, 3]$ relative to the judgment year t is equal to coefficient $\beta_k^{proworker}$ of equation (2.5). The left top panel reports the results for all firms under 100 employees and the right top panel for firms under 100 employees whose return on assets is below the median. The bottom panel provides similar graphs for firms with less than 10 employees. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.4 – Judge pro-worker bias and firm performance 1 year after the judgment

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 1$]	growth rate between $t - 1$ and $t + 1$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.001 (0.001)	-0.009 (0.006)	-0.003 (0.007)	0.001 (0.017)	-0.000 (0.002)	-0.007 (0.005)
R^2	0.025	0.037	0.037	0.030	0.032	0.036
Pro-worker bias × Low Roa	0.000 (0.002)	-0.018** (0.008)	-0.005 (0.010)	-0.019 (0.022)	0.005 (0.003)	-0.014* (0.007)
Pro-worker bias × High Roa	-0.003 (0.002)	-0.000 (0.010)	-0.001 (0.010)	0.020 (0.028)	-0.005 (0.004)	-0.001 (0.007)
R^2	0.025	0.037	0.037	0.030	0.033	0.037
# obs	4486.000	4486.000	4112.000	4112.000	4112.000	4418.000

Note: This table displays the estimates of the correlation between the judge bias and indicators of firm performance. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 1 year after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 1$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 1$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 1$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 1$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 1$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.5 – Judge pro-worker bias and firm performance 2 years after the judgment

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 2$]	growth rate between $t - 1$ and $t + 2$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.003 (0.003)	-0.015** (0.008)	-0.014 (0.009)	0.008 (0.017)	-0.004 (0.004)	-0.017** (0.007)
R^2	0.035	0.043	0.037	0.026	0.033	0.030
Pro-worker bias × Low Roa	-0.007* (0.004)	-0.033** (0.011)	-0.024** (0.012)	0.000 (0.024)	-0.004 (0.005)	-0.030*** (0.009)
Pro-worker bias × High Roa	0.001 (0.004)	0.001 (0.012)	-0.005 (0.014)	0.015 (0.024)	-0.005 (0.006)	-0.005 (0.010)
R^2	0.035	0.044	0.037	0.026	0.033	0.031
# obs	4486.000	4486.000	4112.000	4112.000	4112.000	4395.000

Note: This table displays the estimates of the correlation between the judge bias and indicators of firms performance. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 2 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 2$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 2$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 2$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 2$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 2$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.6 – Judge pro-worker bias and firm performance 3 years after the judgment

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.007** (0.003)	-0.015* (0.009)	-0.018* (0.010)	0.001 (0.022)	-0.008* (0.005)	-0.023** (0.008)
R^2	0.044	0.046	0.046	0.031	0.039	0.028
Pro-worker bias × Low Roa	-0.010** (0.005)	-0.035*** (0.010)	-0.031** (0.012)	-0.005 (0.023)	-0.009 (0.006)	-0.047*** (0.011)
Pro-worker bias × High Roa	-0.004 (0.004)	0.003 (0.012)	-0.006 (0.014)	0.006 (0.033)	-0.007 (0.006)	-0.001 (0.012)
R^2	0.044	0.047	0.046	0.031	0.039	0.030
# obs	4486.000	4486.000	4112.000	4112.000	4112.000	4398.000

Note: This table displays the estimates of the correlation between the judge bias and indicators of firms performance. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

The drop is economically significant: a one standard deviation increase in judge pro-worker bias reduces employment growth by 1.8 percentage points. Low-performing firms also face a drop in their sales growth of the same order of magnitude. By contrast, the overall employment and the sales of high-performing firms defined as those whose returns on assets are above the median, are not significantly impacted by judge bias.

The effects of judge bias become stronger two and three years after the judgment, as shown by Tables 2.5.5 and 2.5.6. They are statistically significant for firms taken as a whole, but they are still entirely driven by low-performing firms which are more seriously affected by judge pro-worker bias as time elapses. The impact on low-performing firms employment is approximately doubled in the third year, compared with the first year. It is striking that the employment effects are induced by the drop in permanent jobs only. Temporary jobs are not affected. All in all, pro-worker bias on the part of judges reduces employment growth and raises its instability.

The effect of judge bias on sales is also more important 3 years after the judgment than one year after. The difference is significant: a one standard deviation increase in judge pro-worker bias reduces sales growth by 1.4 percentage points one year after the judgement and by 4.7 percentage points 3 years after.

Two years after the judgement, judge pro-worker bias has a significant impact on the

Table 2.5.7 – Judge pro-worker bias and firm performance 3 years after the judgment - conditional on surviving

	(1)	(2)	(3)	(4)	(5)
	growth rate between t-1 and t+3				
	Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.002 (0.006)	-0.003 (0.007)	0.008 (0.022)	-0.000 (0.004)	-0.009* (0.006)
R^2	0.040	0.038	0.033	0.027	0.029
Pro-worker bias \times Low Roa	-0.016** (0.008)	-0.013 (0.008)	0.006 (0.025)	0.000 (0.004)	-0.027** (0.008)
Pro-worker bias \times High Roa	0.011 (0.009)	0.006 (0.012)	0.010 (0.033)	-0.001 (0.005)	0.006 (0.009)
R^2	0.041	0.038	0.033	0.027	0.030
# obs	4149.000	3797.000	3797.000	3797.000	4062.000

Note: This table displays the estimates of the correlation between the judge bias and indicators of firms performance for surviving firms. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (4) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (5) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

survival rate of low-performing firms, which drops by 0.7 percentage points two years after the judgment and by 1 percentage point three years after, when the pro-worker judge bias increases by one standard deviation. High-performing firms are not impacted at any time horizon. Interestingly, the employment effects of the judge pro-worker bias within a 3-year horizon are not solely driven by firm death. Table 2.5.7 shows that judge pro-worker bias has a significant negative impact on the growth rate of employment and sales of low-performing firms which survive 3 years after the judgment. Though the selection of this sub-sample is endogenous, it is still informative about the channels at play.

The effects of judge bias on the number of entries and exits are non significant for either type of firms at any time horizon, as shown by Tables 2.5.4, 2.5.5 and 2.5.6. The absence of significant impact is the consequence of two counteracting effects. First, the pro-worker bias reduces employment, which negatively affects the entries and exits. Second, the pro-worker bias decreases the share of permanent jobs, which increases the job turnover. The composition of these two effects induces no significant change in entries and exits in our empirical context.

Small firms, below 10 employees, versus medium-sized firms

One might expect small firms to be more impacted than medium-sized firms by judge bias because the dismissal compensations represent a larger share of the payroll of small firms,

Table 2.5.8 – Judge pro-worker bias and firm performance 3 years after the judgment in small firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.016** (0.007)	-0.016 (0.016)	-0.027 (0.018)	-0.012 (0.025)	-0.017** (0.007)	-0.024 (0.017)
R^2	0.058	0.059	0.061	0.069	0.064	0.053
Pro-worker bias × Low Roa	-0.027** (0.011)	-0.064** (0.022)	-0.058** (0.024)	-0.022 (0.039)	-0.023** (0.011)	-0.063** (0.021)
Pro-worker bias × High Roa	-0.005 (0.007)	0.030 (0.020)	0.004 (0.025)	-0.001 (0.033)	-0.011 (0.010)	0.012 (0.024)
R^2	0.060	0.063	0.062	0.069	0.064	0.057
# obs	1902.000	1902.000	1750.000	1750.000	1750.000	1893.000

Note: This table displays the estimates of the correlation between the judge bias and indicators of firms performance for the sample of small firms, below 10 employees the year before the judgment. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs, in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

and small firms might also be more financially fragile. This is what clearly arises in our context. Table 2.5.8 shows that firms with less than 10 employees are very strongly impacted if their return on assets is below the median at the judgment date. For those firms, a one standard deviation increase in the judge's pro-worker bias reduces employment growth and sales by 6 percentage points at the 3-year horizon. This impact is about twice as high as for all low-performing firms below 100 employees. All employment effects are driven by the drop in permanent jobs, while the number of temporary jobs does not change significantly. High-performing firms, even if they have less than 10 employees, are not significantly impacted by judge bias.

Table 2.5.9 shows that the employment of firms with 10 employees and more is not significantly impacted by judge bias even if they are low-performing firms. The judge bias has an impact of their sales if their return of assets is below the median, which is about half of that estimated for small low-performing firms.

The survival of small low-performing firms is also strongly impacted by the judge pro-worker bias. A one standard deviation increase in the judge pro-worker bias reduces the survival rate by 3 percentage points for low-performing firms at the 3-year horizon. The survival rate of high performing firms, even if they have fewer than 10 employees, is not

Table 2.5.9 – Judge pro-worker bias and firm performance 3 years after the judgment in medium-sized firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	0.001 (0.003)	-0.014 (0.011)	-0.011 (0.010)	0.000 (0.031)	0.000 (0.005)	-0.020** (0.009)
R^2	0.065	0.082	0.079	0.051	0.060	0.054
Pro-worker bias × Low Roa	0.002 (0.006)	-0.017 (0.015)	-0.014 (0.015)	-0.001 (0.033)	0.002 (0.007)	-0.036** (0.015)
Pro-worker bias × High Roa	-0.001 (0.004)	-0.011 (0.013)	-0.007 (0.014)	0.001 (0.046)	-0.001 (0.005)	-0.004 (0.010)
R^2	0.065	0.082	0.079	0.051	0.060	0.055
# obs	2581.000	2581.000	2359.000	2359.000	2359.000	2502.000

Note: This table displays the estimates of the correlation between the judge bias and indicators of firms performance for the sample of medium-sized firms, with 10 to 100 employees the year before the judgment. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

significantly impacted by judge bias.

Overall, it is clear that judge bias has a significant impact on small and low-performing firms, below 10 employees. The judge bias has no significant effects on the employment of larger firms. Firms with return on assets above the median are not significantly impacted by judge bias.

IV estimates

In order to quantify the effect of the amount of compensation for wrongful dismissal induced by judge bias on the outcomes of firms, it is useful to regress the firm outcomes on the amount of compensation for wrongful dismissal, expressed in share of the payroll in the year preceding the judgment, and to instrument this variable by the judge bias. The exclusion restriction is satisfied if the allocation of cases to judges is random, which is arguably the case in our context, as shown above. Table 2.5.10, which reports the results of the first stage of IV estimations, confirms that judge bias is strongly correlated with the share of compensation for wrongful dismissal in the firm payroll.

Table 2.5.11, which reports the results of the second stage of the IV estimations for all

Table 2.5.10 – First-stage IV estimates

	(1)	(2)	(3)	(4)
	All size	All size	Small firms	Small firms
All Roa	1.910*** (0.262)		2.267*** (0.500)	
Low Roa		1.595*** (0.385)		2.393*** (0.866)
High Roa		2.208*** (0.457)		2.924*** (0.928)
R^2	0.064	0.064	0.075	0.075
F	15.79	13.17	6.01	5.05
# obs	4486	4486	1902	1902

Note: This table presents the first-stage estimates of the IV regression where the share of total compensation for wrongful dismissal in the payroll of the year preceding the judgment is instrumented by the judge fixed-effect. Each cell corresponds to one regression where the dependent variable is the share of total compensation for wrongful dismissal in the payroll of the year preceding the judgment. Column (1) displays the results for all firms with less than 100 employees on 31 December before the judgment year; Column (2) for firms with less than 100 employees on 31 December before the judgment year with either low (below the median) or high return on assets; Column (3) and (4) display similar results for firms with less than 10 employees on 31 December before the judgment year. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%, are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

firms below 100 employees, shows that an increase in the amount of compensation of one percent of the payroll reduces employment by 3 percentage points at the 3-year horizon for low-performing firms. The effect arises from the growth of permanent employment, while temporary employment is not significantly impacted. Sales growth is significantly impacted: an increase in the amount of compensation of one percent of the payroll reduces sales growth by 4 percentage points at the 3-year horizon for low-performing firms. High performing firms are not impacted by the shock on their revenue induced by judge bias.

Table 2.5.12 shows that the point estimates reported for all firms below 100 employees are the same as for firms below 10 employees. This means that a transitory shock on the revenue of firms equal to one percent of their payroll has a similar impact on small and medium-sized firms. Hence, the stronger employment impact of pro-worker judges on small low-performing firms found in the reduced form estimates is merely the consequence of the fact that dismissal compensations represent a higher share of the payroll for small firms, below 10 employees, than for medium-sized firms, as shown by Tables 2.5.2 and 2.5.3. This indicates that the same amount of compensation for wrongful dismissal has effects which are very different according to the financial capacity of firms, which is determined by their size and their return on assets. Smaller low-performing firms are likely to be more impacted because of a weaker financial capacity.

In these circumstances, it can be argued that pro-worker bias on the part of judges has cleansing effects by destroying the structurally weakest firms. It cannot be excluded that

Table 2.5.11 – Second-stage IV estimates of the effects of total compensations for wrongful dismissal on firm performance 3 years after the judgment

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Total amount	-0.005** (0.002)	-0.011* (0.006)	-0.013* (0.007)	0.000 (0.015)	-0.006* (0.003)	-0.016** (0.005)
Total amount × Low Roa	-0.008 (0.005)	-0.031** (0.013)	-0.031** (0.015)	0.011 (0.023)	-0.009 (0.007)	-0.041** (0.014)
Total amount × High Roa	-0.003 (0.002)	0.002 (0.007)	-0.004 (0.007)	-0.001 (0.017)	-0.004 (0.003)	0.000 (0.006)
# obs	4486.000	4486.000	4112.000	4112.000	4112.000	4398.000

Note: This table displays the second stage of the IV estimates where Total amount, which corresponds to the share of total compensation for wrongful dismissal in the total payroll of the year preceding the judgment, is instrumented by the judge bias. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.12 – Second-stage IV estimates of the effects of total compensations for wrongful dismissal on performance of firms below 10 employees 3 years after the judgment

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Total amount	-0.008** (0.004)	-0.008 (0.008)	-0.013 (0.009)	-0.006 (0.012)	-0.008** (0.004)	-0.012 (0.009)
Total amount × Low Roa	-0.014* (0.008)	-0.030* (0.016)	-0.031 (0.019)	0.004 (0.022)	-0.014 (0.009)	-0.036*** (0.018)
Total amount × High Roa	-0.001 (0.003)	0.014 (0.009)	0.004 (0.008)	0.003 (0.014)	-0.003 (0.004)	0.007 (0.010)
# obs	1904.000	1904.000	1752.000	1752.000	1752.000	1895.000

Note: This table displays the second stage of the IV estimates where Total amount, which corresponds to the share of total compensation for wrongful dismissal in the total payroll of the year preceding the judgment, is instrumented by the judge bias. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

judge bias improves overall efficiency, since the jobs destroyed by pro-worker judges in low-performing firms might be reallocated at low cost to high performing firms. Addressing this question is left for future research.

2.5.4 The effects of the dispersion of judges bias

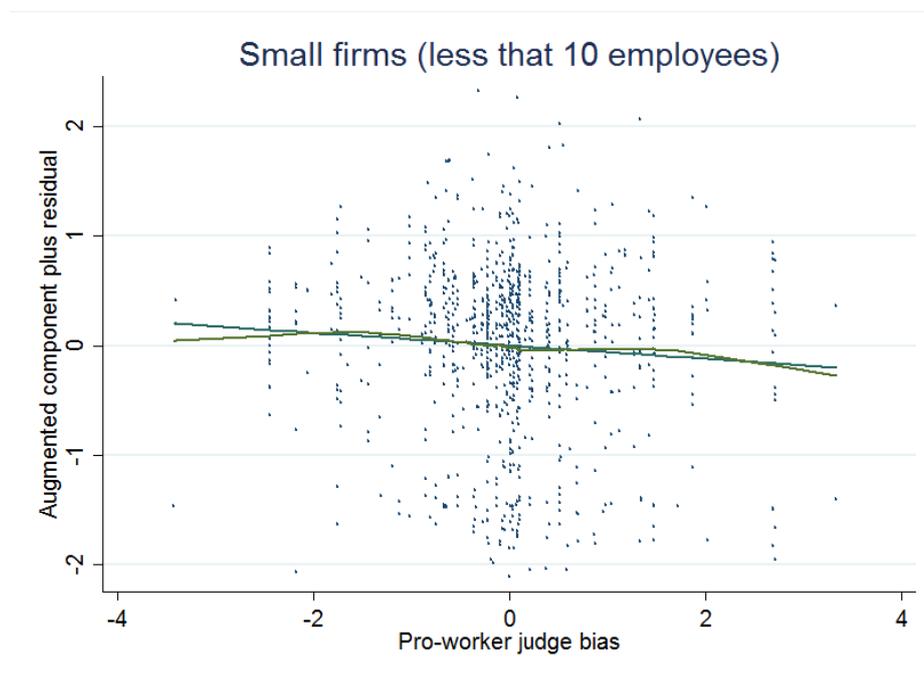
So far, we have uncovered the effects of judge bias on firms survival, employment and sales. A natural question that arises is what would the outcomes be if the dispersion of biases was reduced. Because our measure of bias is relative, setting all our bias estimates to the mean produces the effect of eliminating any judge-related dispersion in dismissal compensation.

It is well known that less uncertainty about judge decisions reduces the litigation rate but also affects the composition of the set of cases going to litigation (Priest and Klein (1984), Lee and Klerman (2016)). Accordingly, the set of cases going to Appeal courts would change if the dispersion of judge bias changed. However, to the extent that the dispersion of judge bias explains less than 0.3% of the variance of compensations conditional on observable worker and firm characteristics, as shown by Table 2.4.3, it is likely that the dispersion of judge bias has negligible effects on the selection of cases that go to Appeal court. Indeed, an approximation of the relative risk premium associated with the dispersion of judge bias implies that an upper bound of the cost of the risk associated with the dispersion of judge bias is at most equal to 1.5% of the average compensation, depending on the degree of risk aversion (see Appendix 2.7.4). This means that the actual dispersion of judge bias has a very limited impact on the selection of cases going to the Appeal courts. Hence, in what follows, we evaluate the consequences of reductions in the dispersion of judge bias for our sample of firms which go to Appeal courts assuming that such changes in the dispersion of judge bias have negligible selection effects.

First, changes in the mean-preserving spread of judges biases can have an effect on the mean outcome of firms only if the bias of judges has non-linear effects on firm outcomes. Therefore, we start by analyzing whether judge bias has non-linear effects on firm outcomes. It is indeed plausible that judges with a strong pro-worker bias who set very high compensation for wrongful dismissal have a disproportionately strong impact, especially on small, low-performing firms.

Focusing on small firms below 10 employees whose return on assets is below the median, which are the only firms for which judge bias has a significant impact, we do not find any evidence of non-linearity, either from visual inspection of augmented component-plus-residual plots (see Figure 2.5.5), or from the introduction of quadratic terms in the reduced form

Figure 2.5.5 – Augmented component-plus-residual plot



Note: This figure is an augmented component-plus-residual plot of the reduced form estimation of the correlation between the judge bias and the employment growth of firms with fewer than 10 employees and whose return on assets is below the median at 3-year horizon displayed in Table 2.5.8. The non-linear line is a loess smooth of the plotted points. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

equations (see Table 2.5.13). This means that mean-preserving spread changes in judge bias have no significant impact on the average outcome of firms potentially impacted by judge bias.

Then, we perform counterfactual exercises in which we cap judge bias at several percentiles of the distribution of bias. To do so, we first estimate, based on 1,000 bootstrap replications, the predicted outcomes for small low-performing firms from equation (2.6) on samples featuring counterfactual distributions of bias to obtain the counterfactual distributions of predicted outcomes. We perform similar bootstrap replications based on our initial sample of small low-performing firms, to obtain a distribution of predicted outcomes with the actual distribution of judge bias. Figure 2.5.6 reports the mean and the 95% confidence interval of the differences between those predicted outcomes three years after the judgments for different counterfactual distributions. It is clear that reducing the dispersion of judge bias has very small and non-significant effects on firm survival and employment growth. Confirming our observation of absence of non-linear effects of judge bias on firm outcomes, Figure 2.5.6 shows that setting all biases to the mean yields point estimates for the difference between the actual and the counterfactual outcomes very close to zero, and these point estimates are not significantly different from zero at any standard confidence level. Capping the bias of pro-worker judges to the mean has a larger impact, but one which remains small and far from statistically

Table 2.5.13 – Judge pro-worker bias and small low-performing firm performance 3 years after the judgment, with quadratic terms

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.026** (0.012)	-0.058** (0.024)	-0.055** (0.026)	0.005 (0.041)	-0.025** (0.012)	-0.066** (0.023)
Pro-worker bias ²	-0.004 (0.006)	0.009 (0.014)	0.004 (0.015)	0.008 (0.029)	-0.004 (0.008)	0.006 (0.015)
R^2	0.134	0.111	0.113	0.109	0.135	0.104
# obs	973.000	973.000	911.000	911.000	911.000	966.000

Note: This table displays estimates of the correlation between the judge bias and indicators of firms performance for firms below 10 employees whose return on assets is below the median the year preceding the judgment. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

significant. The same result arises when the bias of pro-employer judges is capped to zero.

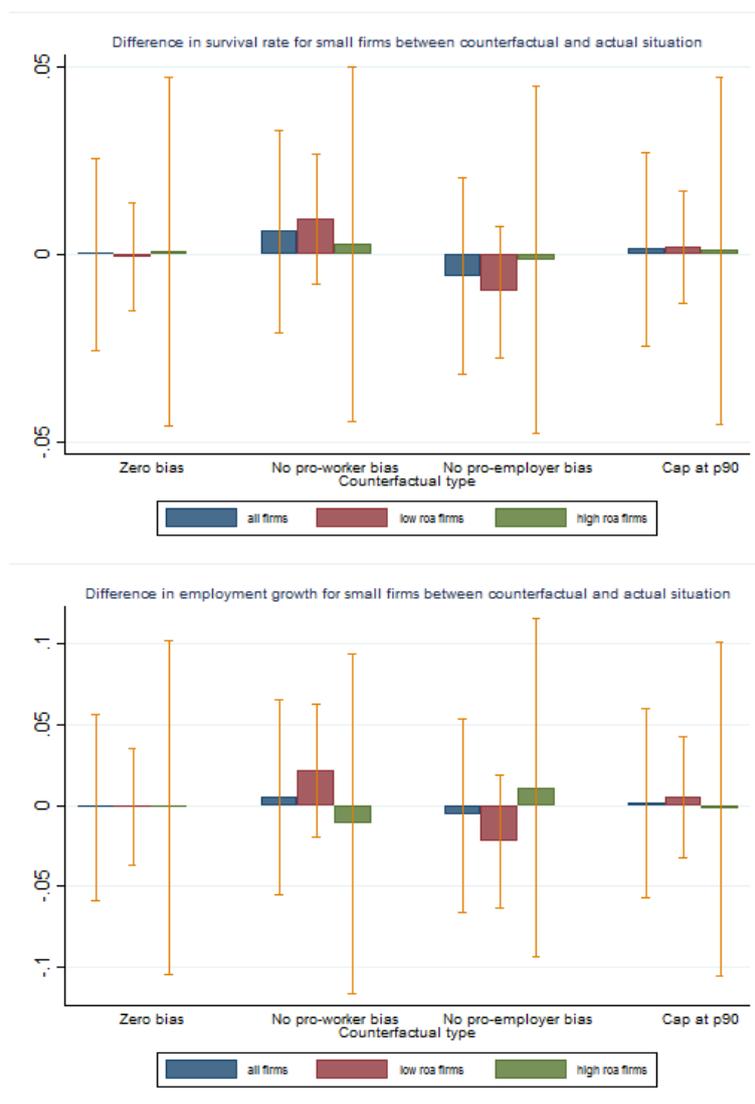
These findings clearly indicate that capping or reducing the dispersion of judge bias has very limited effects on firms, even for small, low-performing firms which are the most impacted by judge bias. An open question that our study cannot address, however, is the possibility that all judges are biased, meaning that setting all biases to the mean does not ensure the absence of bias in the interpretation of labor laws (see: Ash et al. (2018)).

2.5.5 Robustness checks

We conduct a range of checks both to test the robustness of the previous results and to investigate the mechanisms at play.

First, we conduct placebo tests for the significance of the effect of judge bias on firm performance before the judgment. By definition, we cannot proceed to placebo tests on firm survival before the judgment since all firms which are judged by Appeal courts necessarily survive until the date of the judgment. In this context, placebo tests are similar to regressions run on surviving firms, presented in Table 2.5.7, which reports negative significant correlations between the pro-worker bias of judges and employment and sales growth. Table 2.5.14 documents the absence of significant correlation between judge bias and the growth rates of these variables between two years and one year before the judgment for all firms and for small firms, whether their are high-performing or low performing firms. This means that the effects of judge bias

Figure 2.5.6 – Counterfactual exercises



Note: This figure reports the results of counterfactual exercises in which we cap judge bias at several percentiles of the distribution of bias. To do so, we first estimate the predicted outcomes of firms from the estimation of equation (5) applied to our initial sample with 1,000 bootstrap replications. This yields a distribution of predicted outcomes with the actual distribution of judge bias. Then, we repeat the same exercise for the samples with counterfactual distributions of the judge bias to obtain the distributions of predicted outcomes with counterfactual distributions of judge bias. The top panel and the bottom panel respectively report the mean and the 95% confidence interval of the differences between those predicted outcomes three years after the judgments for the survival rate and the employment growth rate at 3 year-horizon for firms with fewer than 10 employees whose return on assets is below the median the year preceding the judgment. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.14 – Placebo tests: Judge pro-worker bias and firm performance before the judgment

	(1)	(2)	(3)	(4)	(5)
	growth rate between t-2 and t-1				
	Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
	All firms				
Pro-worker bias × Low Roa	-0.005 (0.007)	-0.001 (0.008)	-0.004 (0.021)	0.002 (0.005)	-0.001 (0.006)
Pro-worker bias × High Roa	-0.005 (0.005)	-0.002 (0.010)	0.036 (0.023)	0.003 (0.009)	0.006 (0.005)
R^2	0.039	0.047	0.034	0.042	0.035
# obs	4282	3420	3420	3420	4224
	Small firms				
Pro-worker bias × Low Roa	-0.001 (0.011)	-0.008 (0.013)	-0.040 (0.028)	-0.005 (0.009)	0.000 (0.008)
Pro-worker bias × High Roa	-0.005 (0.010)	-0.0028 (0.020)	0.050 (0.034)	- 0.014 (0.017)	0.009 (0.010)
R^2	0.083	0.089	0.067	0.068	0.075
# obs	1843	1477	1477	1477	1829

Note: This table displays the estimates of the correlation between the judge bias and indicators of firm performance. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) the symmetric growth rate between $t - 2$ and $t - 1$ of firm's employment; in Column (2) the symmetric growth rate between $t - 2$ and $t - 1$ of firm's employment in permanent contract - *cdi*; in Column (3) the symmetric growth rate between $t - 2$ and $t - 1$ of firm's employment in temporary contract; in Column (4) the change between $t - 2$ and $t - 1$ in the share of permanent jobs; in Column (5) the symmetric growth rate between $t - 2$ and $t - 1$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roa firms denote firms with a return on assets below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The table displays coefficient coefficients β_1 and β_2 of equation (2.7) for all firms in the top panel and for firms with less than 10 employees the year before the judgment. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

on firm performance after the judgment year which are identified by our empirical strategy are not driven by selection of firms due to the anticipation of judge bias.

Second, the effects of judge bias we find are significant only for low-performance firms – defined as firms with a below-median return on assets. One may wonder whether this result would hold for different measures of the financial situation of firms. In order to investigate this issue, Table 2.5.15 contrasts the effect of judge bias according to the level of return on equity. By definition, the return on equity of high performing firms is above the median and that of low-performing firms is below the median. The bias of judges has a significant impact on low-performing firms only and the effect is larger for small low-performing firms, which confirms the results obtained when the performance of firms is measured with the return on assets.

Third, we examine the results for the sub-sample of cases which go to large Appeal courts that contain several social chambers, because, as explained above in Section 2.5.2, it is even more likely that the parties do not know until the day of the judgment the identity of the president who will be in charge of the case when there are several social chambers. These large Appeal courts, located at Aix-en-Provence, Paris and Versailles, have 4, 14 and 7 social

Table 2.5.15 – Judge pro-worker bias and firm performance 3 years after the judgment - according to return on equity

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
	growth rate between $t - 1$ and $t + 3$					
All firms						
Pro-worker bias	-0.011**	-0.037**	-0.043***	-0.005	-0.013**	-0.047***
× Low Roe	(0.004)	(0.011)	(0.012)	(0.029)	(0.006)	(0.011)
Pro-worker bias	-0.003	0.010	0.010	0.008	-0.001	-0.002
× High Roe	(0.005)	(0.012)	(0.015)	(0.028)	(0.007)	(0.011)
R^2	0.044	0.047	0.046	0.031	0.039	0.030
# obs	4447	4447	4084	4084	4084	4369
Small firms						
Pro-worker bias	-0.025***	-0.062**	-0.087***	-0.036	-0.028**	-0.063***
× Low Roe	(0.007)	(0.021)	(0.020)	(0.038)	(0.010)	(0.017)
Pro-worker bias	-0.003	0.036	0.038	0.014	-0.002	0.019
× High Roe	(0.010)	(0.023)	(0.032)	(0.030)	(0.013)	(0.023)
R^2	0.054	0.057	0.061	0.069	0.061	0.058
# obs	1887	1887	1735	1735	1735	1878

Note: This table displays the estimates of the correlation between the judge bias and indicators of firm performance. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roe firms denote firms with a return on equity below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 2.5.16 – Judge pro-worker bias and firm performance 3 years after the judgment - Large Appeal courts

	(1)	(2)	(3)	(4)	(5)	(6)
	Survival within [$t, t + 3$]	growth rate between $t - 1$ and $t + 3$				
		Employment	Employment <i>cdi</i>	Employment <i>cdd</i>	Share <i>cdi</i>	Sales
Pro-worker bias	-0.012**	-0.035***	-0.030**	-0.039*	-0.006	-0.028**
	0.050	0.058	0.060	0.050	0.053	0.044
R^2	0.050	0.058	0.060	0.050	0.053	0.044
Pro-worker bias	-0.010	-0.038***	-0.029**	0.007	-0.005	-0.024*
× Low Roa.	(0.007)	(0.010)	(0.013)	(0.026)	(0.009)	(0.012)
Pro-worker bias	-0.014***	-0.032**	-0.030*	-0.084**	-0.006	-0.032*
× High Roa.	(0.004)	(0.013)	(0.015)	(0.025)	(0.008)	(0.018)
R^2	0.050	0.058	0.060	0.052	0.053	0.044
# obs	2074	2074	1907	1907	1907	2022

Note: This table displays the estimates of the correlation between the judge bias and indicators of firm performance in large Appeal courts which comprise several social chambers. t denotes the year of the Appeal court judgment. The dependent variable is in Column (1) an indicator variable equal to one if the firm survives 3 years after the judgment; in Column (2) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment; in Column (3) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in permanent contract - *cdi*; in Column (4) the symmetric growth rate between $t - 1$ and $t + 3$ of firm's employment in temporary contract; in Column (5) the change between $t - 1$ and $t + 3$ in the share of permanent jobs; in Column (6) the symmetric growth rate between $t - 1$ and $t + 3$ of sales. The variable of interest is the judge pro-worker bias computed as defined in section 2.4.2. Low roe firms denote firms with a return on equity below the median the year before the judgment. Covariates include Appeal court fixed effects, year fixed effects, the leave-one-out average industry annual growth rate of sales and an indicator variable for economic dismissals. The upper part of the table displays coefficient α_1 of equation (2.6) and the bottom part coefficients β_1 and β_2 of equation (2.7). Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

chambers respectively. Although the number of observations is about half that of the whole sample, Table 2.5.16 shows that we get similar results when the sample is restricted to large Appeal courts. This confirms that our results are not driven by non-random allocation of cases to judges.

2.6 Conclusion

Using new data on Appeal court rulings about dismissals merged with firm data, this chapter provides the first systematic analysis of the impact of judge bias on dismissal compensation and on firm performance. It shows that the subjective opinion of judges influences the amount of dismissal compensation: some judges appear more likely to rule in favor of the employer and others in favor of dismissed workers. We find that the bias of judges has a significant impact on employment, sales and survival of small firms, especially very small and low performing ones, hence partly confirming the intuition of policy makers who implemented reforms to limit the power of judges in the setting of dismissal compensation. However, the actual dispersion of judge bias, before the implementation of such reforms in France, does not seem to have had significant detrimental effects on the average performance of firms going to Appeal courts,

even the weakest and the smallest ones. The main reason is that the risk premium associated with the dispersion of judge bias is very small compared with the expected amount of dismissal compensation.

It is worth stressing that our paper does not fully address the question of the impact of judge bias on overall employment. It may be that the publicity around several extreme cases, with very high compensations, has a strong impact on the beliefs of employers and thus on hiring behavior and firm entry. It is also possible that cases judged by Appeal courts are not representative of all cases. From this perspective, our paper must be completed by future research to better understand the effects of judge bias on employment, firm creation and destruction.

2.7 Appendices

2.7.1 Appendix A: Caps on dismissal compensation in European countries

A majority of European countries have set rules that limit the amounts granted by judges in case of unfair dismissal (excluding cases of discrimination or harassment):

- In **Italy**, a fixed amount compensating an unfair dismissal was introduced in 2014 by the so-called (*Jobs Act*) for the new indefinite-duration contract with progressive employment protection, which depends on seniority: from 4 months for less than 2 years of seniority to 24 months for 12 years of seniority. From these amounts one must deduce the compensation received at the time of dismissal. In 2018 the Italian Constitutional Court overruled this regulation, stating that the amount of compensation to the worker cannot be based only on her seniority.
- In **Germany** the schedule depends on seniority and reaches 12 months of salary (and even 15 months if the worker is more than 50 years old with more than 15 years of seniority, and 18 months if more than 55 years old with more than 20 years of service).
- In **Austria**, the schedule depends on seniority: for those with less than 2 years the amount is 6 weeks of salary; between 2 and 5 years it is 2 months; between 5 and 15 years, 3 months; between 15 and 25 months, 4 months; beyond that: 5 months of salary.
- In **Belgium**, the minimum compensation is 3 weeks and the maximum 17 weeks of salary.
- In **Denmark**, worker compensation is capped at 1 year of salary for blue-collar; for white-collar workers, compensation goes up to half of the wages received during the notice period, capped at 3 months for those under 30, at 4 months if more than 10 years of service and 6 months if they have more than 15 years of service.
- In **Spain**, the indemnity is set at 33 days per year of seniority with a maximum of 24 months of salary, for contracts signed since the 2012 labor market reform.
- In **Finland**, the allowance is between 3 (minimum) and 24 (maximum) months of salary, depending on several factors including seniority, the age of the employee, the length of unemployment period, or the loss of income.

- In the **Netherlands**, the schedule depends above all on the age of the employee (1/2 month of salary per year of seniority up to 35 years old, 1 month per year of seniority between 35 and 45 years old, 1.5 month per year of seniority between 45 and 55 years old, 2 months per year of seniority beyond 55), to which a correction factor can be added depending on the exact situation. From these amounts one must deduce the compensation received at the time of dismissal.
- In **Portugal**, the court may grant between 15 (minimum) and 45 (maximum) days of salary per year of seniority with a minimum of 3 months.
- In the **United Kingdom**, for employees with more than two years of seniority the allowance consists of two components (i) a basic allowance which depends on seniority and capped at £ 14,250 and (ii) a compensatory allowance capped at one year of salary and limited to £ 78,335.
- In **Sweden**, the allowance is 16 months of salary for employees with less than 5 years of seniority, 24 months between 5 and 10 years, and 32 months for more than 10 years.
- In **France** since 2017 (*Ordonnances*), compensation for unfair dismissal is capped by an amount that depends on seniority varying from 1 month to 20 months for employees with 30 year or more of tenure, and cannot be less than 3 months of salary for employees with at least 2 years of seniority (at least 11 years for those working in firms with fewer than 11 employees).

2.7.2 Appendix B: Computation of judge bias

To compute the bias of judges, we can estimate

$$y_{ijkt} = \eta_{kt} + \nu_{ijkt} \quad (2.9)$$

Assuming $E(\nu_{ijkt}|\eta_{kt}) = 0$, meaning that the compensation awarded in case i is assumed to be equal to a term common to all cases judged in the same chamber and year as case i plus a random term. This implies that the chamber \times year fixed effect in chamber k in year t is defined by the expectation of the compensation y_{ijkt} in chamber k in year t :

$$\eta_{kt} = E(y_{ijkt}|k, t) \quad (2.10)$$

the sample counterpart of which is

$$\hat{\eta}_{kt} = \frac{1}{n_{kt}} \sum_{i \in (k,t)} y_i \quad (2.11)$$

where $i \in (k, t)$ stands for all the cases judged in chamber k at date t and n_{kt} is the number of cases judged in chamber k in year t . The chamber k fixed effect in year t is equal to the average of all compensations in chamber k in year t .

By definition, the estimator of the judge fixed effect, conditional on the chamber \times year fixed effect is

$$\hat{\varepsilon}_j = \frac{1}{n_j} \sum_{i \in j} \hat{v}_i \quad (2.12)$$

where $i \in j$ stands for all cases i judged by judge j . Let us denote by $(K, T)(j)$ the set of all chamber \times year pairs (k, t) observed for judge j . From equations (2.9) and (2.12), we can write

$$\hat{\varepsilon}_j = \frac{1}{n_j} \sum_{i \in j} y_i - \frac{1}{n_j} \sum_{(k,t) \in (K,T)(j)} \frac{n_{jkt}}{n_{kt}} \hat{\eta}_{kt} \quad (2.13)$$

Equation (2.13) shows, together with equation (2.11), that $\hat{\varepsilon}_j$ is equal to $\bar{\varepsilon}_j$ defined in equation (2.2).

2.7.3 Appendix C: Judge mobility and judge ranking

To illustrate the relation between the mobility of judges and their ranking according to their bias, suppose a simple situation with one period only and four judges, A, B, C, D , ranked from the least to the most (unknown) pro-worker bias. Suppose that A and D belong to the same social chamber and that C and B belong to another social chamber during the whole period. Our measure of the bias relies on the difference in the share of dismissals deemed wrongful by different judges belonging to the same social chamber with respect to the average share of dismissals deemed wrongful in that social chamber. It allows us to conclude that D is more pro-worker than A and that C is more pro-worker than B . But it yields information neither about the comparison of B and A nor about the comparison of D and C because the average share of dismissals deemed wrongful in the social chamber is different, and depends, among other factors, on the true bias of judges allocated to the social chamber. Depending on the selection of judges in social chambers according to their bias, we may conclude that the ranking is (by increasing order of pro-worker bias) B, A, C, D , or B, C, A, D or A, D, B, C instead of the true ranking A, B, C, D . In our approach, this problem is mitigated insofar as

judges are mobile across social chambers. In the previous example, A might, during the period of interest, share the same social chamber as both D and B , which may enable us to rank A, B and A, D . Such judge mobility thus may help us to exclude the erroneous rankings B, A, C, D and B, C, A, D . Hence, the higher the degree of judge mobility, the higher the probability to achieve a perfect ranking.

2.7.4 Appendix D: Risk premium associated with the bias of judges

This appendix presents the computation of the approximation of the risk premium associated with the bias of judges following standard treatment of the computation of risk premium (see e.g. Eeckhoudt, Gollier and Schlesinger (2005)).

Let us consider a lottery which yields $w(1 + e)$, where w is a fixed amount and e is a random variable, whose expected value is $\mathbb{E}(e) = 0$. Let us denote by u the von Neumann and Morgenstern utility function. The relative risk premium π associated with the random term e is defined by

$$\mathbb{E}(u[w(1 + e)]) = u[w(1 - \pi)] \quad (2.14)$$

First order approximation of $u[w(1 - \pi)]$ in the neighborhood of $\pi = 0$ yields:

$$u[w(1 - \pi)] \simeq u(w) - \pi w u'(w)$$

Second order approximation of $\mathbb{E}[w(1 + e)]$ in the neighborhood of $e = 0$ yields:

$$\begin{aligned} \mathbb{E}(u[w(1 + e)]) &\simeq \mathbb{E}\left[u(w) + weu'(w) + \frac{1}{2}(we)^2 u''(w)\right] \\ &= u(w) + w\mathbb{E}(e)u'(w) + \frac{1}{2}\mathbb{E}((we)^2) u''(w) \\ &= u(w) + \frac{w^2}{2}\mathbb{E}(e^2) u''(w) \end{aligned}$$

Substituting these two approximations into equation (2.14), we get

$$\pi \simeq \frac{1}{2}\mathbb{E}(e^2) \rho(w)$$

where $\rho(w) = -\frac{wu''(w)}{u'(w)}$ is the Arrow Prart coefficient of relative risk aversion.

Now, if we consider another lottery, which yields $w(1 + e_1)$, where w is the same fixed amount as in the first lottery and e_1 is a random variable, with $\mathbb{E}(e_1) = 0$, we can compute

the risk premium in the same way and get

$$\pi_1 \simeq \frac{1}{2} \mathbb{E}(e_1^2) \rho(w)$$

The two last equations imply that

$$\pi_1 - \pi \simeq \frac{1}{2} [\mathbb{E}(e_1^2) - \mathbb{E}(e^2)] \rho(w) \quad (2.15)$$

In our setup, e_1 can be defined as the random variable including judge bias and e as the random variable without judge bias. Therefore, $\pi_1 - \pi$ can be interpreted as the risk premium associated with the judge bias.

The variance of total compensation is equal to

$$\mathbb{E}[(w(1 + e_1))^2] - [\mathbb{E}(w(1 + e_1))]^2 = w^2 \mathbb{E}(e_1^2) \quad (2.16)$$

Now, let us assume that the judge biases explain the share λ of the variance of total compensation, i.e.

$$\lambda = \frac{w^2 \mathbb{E}(e_1^2) - w^2 \mathbb{E}(e^2)}{w^2 \mathbb{E}(e_1^2)} = \frac{\mathbb{E}(e_1^2) - \mathbb{E}(e^2)}{\mathbb{E}(e_1^2)}$$

Substituting in (2.15) we get:

$$\pi_1 - \pi \simeq \lambda \frac{1}{2} \rho(w) \mathbb{E}(e_1^2)$$

or, using the expression of $\mathbb{E}(e_1^2)$ given in equation (2.16):

$$\pi_1 - \pi \simeq \lambda \frac{1}{2} \rho(w) \frac{\mathbb{V}[w(1 + e_1)]}{w^2}$$

According to Table 2.4.1, the standard deviation of compensations for wrongful dismissal is equal to 56,385 euros and the mean compensation amounts to 31,461 euros. Table 2.4.3 shows that the dispersion of judges biases explains 0.3% of the variance of compensations for wrongful dismissals. This implies that the risk premium is equal to $(0.003)(1/2)(56385/31461)^2 \approx 0.0048$ times the coefficient of relative risk aversion, whose estimation is between 1 and 3 for workers (see Chetty (2006) and Hendren (2017)) and smaller for firms which have more possibility of risk diversification.

2.7.5 Appendix E: Extraction of compensation amounts and other variables of Appeal court rulings

This section provides additional details on the construction of our novel database of anonymized Appeal court rulings. We use the universe of Appeal court ruling over ten years. The latter are available and digitized on a systematic basis, contrary, to first instance rulings, which are collected locally at the court level and are not compiled in a common legal database. We use Natural Language Techniques (NLP) to extract the information from close to 145,000 text documents. Each of these rulings is a few pages long, with some spreading over a dozen pages. Extracting information accurately from textual documents that contain many digressions and qualitative arguments is not a straightforward exercise. In order to reduce the complexity of the problem, we exploit the structure of these legal documents, which follow a well-established template.

Structure and recognizable information within rulings

Each ruling can naturally be divided into roughly five blocks as follows i) a brief header with the case number, the date of the audience, identities of the parties, etc.; ii) a description of the history of the contractual relationship between the employee and the employer with the parties' claims iii) a restatement of the decision appealed; iv) the main arguments behind the rulings containing the reassessment by the Appeal Court of factual elements and the legal groundings of the first-instance decision; and v) the conclusion ruling whether the dismissal is deemed wrongful, and assigning monetary awards, if any. We split these main blocks by tagging the text with specific legal keywords used to mark the boundaries of the different sections. For instance, the conclusion is generally introduced by the expression "Par ces motifs" (For these reasons) or variants thereof.

We then extract the information from each block and generate up to several hundred variables for any given text. This is because there is a wide array of potential damages that can be sought by the parties and/or awarded. Besides compensation for wrongful dismissal (*indemnité pour licenciement sans cause réelle et sérieuse*), the following compensations may also be awarded by Appeal court judges: compensation for non-respect of the dismissal procedure; compensation for unpaid wages (*indemnité pour rappel de salaire*); compensation for moral and financial damages (*indemnité pour préjudice moral et financier*); compensation in lieu of notice period (*indemnité compensatrice de préavis*) when the notice period was not respected; compensation under article 700 of the French Code of Civil Procedure, which covers the legal costs of the winning party; compensation for unpaid annual leave (*indemnité compensatrice*

de congés payés) ; allowance for overtime hours (*heures supplémentaires*). An employee may receive these different compensations concurrently.

It is important to track compensations along all these dimensions because the amounts granted by judges under these various motives are not fully independent, even though in principle the legal bases for granting them are distinct. In other words, it is possible that in a judge's assessment of the case the amounts become correlated. To detect substitution between the different types of monetary awards, we keep track of all of them using initially more than twenty categories before aggregating them. Because of the length of legal proceedings, some amounts, still expressed in French francs before the adoption of the Euro in 2001, also need to be appropriately converted.

It turns out that judges often award these other types of compensation. They are awarded alongside compensations for wrongful dismissal to workers, but not only that, as rightful dismissal can also be marred by procedural irregularities. In total, out of 145,000 cases in our original sample of court decisions, a positive amount is awarded to workers in 60% of the cases, whatever the motive. Out of these cases receiving a positive amount, the dismissal is deemed unfair 61% of the time. But workers also receive compensation for other reasons, such as paid leave (47% of cases), advance notice (40%), salaries (13%) or overtime hours (7%) when these amounts were due but had not been fully paid by the employer prior to the dismissal. More rarely do judges award compensation for moral damage (2%), harassment (2%) or discrimination (0.3%). One or several of these other types of compensation are awarded in 93% of the cases with a positive amount paid to the worker at the end of the trial.

The data include a wide array of variables related to the case (compensations for wrongful dismissals, worker seniority, wage, Appeal court, city of the *Prud'hommes* council, whether it was the worker who appealed, etc.), as well as the firm's name and address. Using the firm's name and address we are able to retrieve the firm identifier (*SIREN*), and then link the compensation dataset to matched employer-employee data as well as financial variables. The stages for the construction of this dataset are the following.

Extracting wages and tenure requires paying close attention to the wording of rulings as there is substantial heterogeneity in how they are reported. For instance tenure information is sometimes not explicitly stated as a duration but can be recovered from the mentions of when the employee was hired. We therefore use multiple approaches to recover the information. Recovering wages is crucial in order to express the compensation in terms of months of salary. Again, we target a large number of keywords to detect mentions of annual, monthly, weekly, or even hourly wages. Despite our best efforts, for some court rulings the information

could not be fully extracted, thus creating missing observations.

Variable selection and sample attrition

Heterogeneity in the writing of the rulings across jurisdictions and over time means that an automatic extraction can generate mistakes and approximations. Therefore we conducted a series of manual checks on a subsample of 2,560 observations, selected at random. The manual dataset creation was undertaken as part of a project of Pierre Cahuc and Stéphane Carcillo, and funded by the *Chaire sécurisation des parcours professionnels*. To examine the Appeal court rulings published by the Minister of Justice, ten research assistants were hired, each of them being in charge of a given year. These assistants carried out the research with the following key words: ‘*licenciement sans cause réelle et sérieuse*’ (unfair dismissal) and ‘*indemnités*’ (compensation). Even though the research assistants were asked to select randomly Appeal court rulings within the year, some of them selected only rulings from particular months: the assistants in charge of studying the 2009, 2010 and 2012 years mostly selected court rulings of September and October, and marginally court rulings from November and December. We find that the correlation between the compensation amount of the manually-filled and the automatically-filled datasets is equal to 94%, which is in the upper range of seminal papers using this type of approach (Baker et al. (2016)).

Chapter 3

Large firms' collusion in the labor market: Evidence from collective bargaining

This chapter is co-authored with Antoine Valtat.¹

Abstract In several countries, including France, industry-level agreements are binding for all firms of the industry, whether they sit at the negotiating table or not. This chapter provides a theoretical framework showing that dominant firms can use such agreements to reduce competition. In this framework, the higher the over-representation of large firms in employers federations, the larger the bargained wage floors, which entails in turn the eviction of small firms. We test this prediction using French administrative data and document the domination of large firms within federations. We devise an instrumental strategy to causally show that the larger the bargaining firms relative to the other firms of the industry - *i.e.* the lower the federation's representativeness, the higher their incentives to raise wage floors.

Keywords: collective bargaining, representativeness, minimum wage.

JEL classification: J21, J52, J58.

¹I thank Antoine Bertheau, Pierre Cahuc, Francis Kramarz, Frank Malherbet, Pedro Martins, Isabelle Méjean, Arne Uhlendorff for fruitful conversations on this project, as well as all CREST seminar participants. I am indebted to Denis Fougère, Erwan Gautier and Sébastien Roux for providing the data on wage floor negotiations in France. I thank INSEE (Institut National de la Statistique et des Etudes Economiques) and CASD (Centre d'accès sécurisé distant aux données) for providing access to the French data and for continuous technical support. This study also uses the weakly anonymous Establishment History Panel (Years 2003 - 2015). Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and/or remote data access. I thank the Department of Economics of Aarhus University for providing aggregate information on Danish sectors. I acknowledge the financial support from the *Chaire de Sécurisation des Parcours Professionnels*.

3.1 Introduction

Minimum wages arguably constitute one of the most controversial labor market policy and have been the subject of recent debates in the US. Some states, such as California or New York, have increased the minimum wage to \$15/hour. Simultaneously, new empirical evidence showed that the minimum wage has no negative effect on the number of low-wage jobs (see Cengiz, Dube, Lindner and Zipperer (2019)), that disemployment effects are small (see Harasztosi and Lindner (2019)) and that minimum wages reallocate low-wage workers from small to large productive firms (see Dustmann, Lindner, Schönberg, Umkehrer and Vom Berge (2019)). Debates not only center around the optimal amount of minimum wage, but also around the level at which minimum wages should be instituted. In several European countries for instance, collective wage bargaining takes place at the sectoral level, and such a bargaining system has been advanced by several US presidential candidates as a model to copy. To do so, wage boards would bring together businesses and workers to engage in bargaining and determine a minimum wage for a whole industry. We argue in this chapter that the outcome of sectoral-level wage bargaining largely depends on who participates in the negotiation, and we answer the following question: do dominant firms use sectoral-level wage bargaining as an anti-competitive device?

This chapter makes two contributions. Our first contribution is to build a theoretical framework that provides a novel channel *via* which large firms may use collective agreement legislation to reduce competition in the product market: under-representativeness of employers federations. In several countries - among which France, Italy or Portugal - the bargained wages are extended to all firms of the industry, whether they sit at the negotiating table or not. If bargaining firms have different objectives as the average firm in the industry - *i.e.* are *unrepresentative* of the industry, the bargained wage may favour affiliated firms. In particular, the domination of employers federations by large firms² - that we will denote *unrepresentativeness* in the following - , tilts the bargaining process in their favour. This generates a cartel effect insofar as dominant firms can use collective bargaining to raise the labor cost of competitors, and in doing so, reduce the number of producing firms. Our second contribution is to test the model's predictions using novel French data on the representativeness of employers federations.

First, we build a theoretical model inspired by Melitz (2003) in which we compare two different levels of wage bargaining: firm-level and industry-level bargaining. We find that the

²See Mortimer, Bain and Bond (2004) for a case study and Barry and Wilkinson (2011) for a literature review, Traxler (2000))

higher the firm's productivity, the higher the rent to be shared, and therefore the higher the wage negotiated at the firm level. Consequently, the introduction of industry-level wage floors raises the small firms' wages above their optimal level, thus driving them out of the market. The more employers federations are dominated by large firms, the higher the negotiated wage floor and, as a result, the lower the product market concentration.

We provide empirical support for the collusion effect highlighted by the model. We first derive novel stylized facts on the relation between the representativeness of employers federations and the degree of competition of an industry. To measure representativeness, we construct a novel proxy using unique data from the Ministry of Labour. This dataset enables us to compare for the first time the average size, for each industry, of bargaining firms as compared to the average size of all firms of the industry. The index built therefore indicates how likely small firms are to participate in collective wage bargaining, and therefore proxies the domination of employers federations by large firms, *i.e.* the federations' unrepresentativeness. We find a positive correlation between unrepresentativeness and product market concentration, as well as between unrepresentativeness and small firm's destruction rate.

In our model, the mechanism explaining the positive correlation between federations unrepresentativeness and product market concentration is that bargaining firms have higher incentives to raise wage floors the larger they are compared to the average firm of the industry - *i.e.* the more unrepresentative the employers federation. Our model indeed establishes that large firms always have higher incentives than small firms to raise the wage floors because it enables them to evict the small firms from the market. However, for that to translate into higher wage floors, large firms must be over-represented in the bargaining process. Therefore, the over-representation of large firms in employers federations - that we call unrepresentativeness of federations - is a crucial component to understand the outcomes of the bargaining system. In other words, bargaining firms have differential incentives to raise wage floors whether they are representative or not of the average firm in the industry.

However, this mechanism cannot be directly tested because bargaining firms incentives to raise the wage floor are unobservable by nature. We solve this problem by using a variable shifting the large firms incentives to raise wage floors: the share of workers in their industry employed by small firms. The higher the share of workers employed by small firms, the higher the incentives for large firms to increase bargained wage floors. Indeed, the higher this share, the higher the competition from small firms, and thus the higher the large firms incentives to evict small firms from the market.³ If bargaining firms are the largest firms - which occurs in

³A similar argument is used in Magruder (2012).

unrepresentative industries - then the share of workers employed in small firms should have a positive effect on the bargained wage floors. On the opposite, in representative industries, the share of workers employed by small firms should not have any effect on the bargained wage floors. We use our index of unrepresentativeness of the employers federations and estimate in the same regression the effect of the share of workers employed by small firms for both representative and unrepresentative industries on wage floors evolution.

Two variables are likely to be endogenous in our setting: the share of workers employed by small firms for representative industries and the share of workers employed by small firms for unrepresentative industries.⁴ To achieve causality, we thus use two instrumental variables: for each industry, we construct the share of workers employed by small firms in both Denmark and Germany. Our instrumental strategy is based on the assumption that there is no unobserved variable affecting both those foreign shares and French domestic labor costs. Naturally Danish and German sectoral shares are correlated with the industry unobserved comparative advantages through international trade. To mitigate this issue, we exploit the existence of several wage floors per industry agreement and add industry \times year fixed effects, thereby controlling for such comparative advantages. Moreover, we ensure that there is no common shock affecting both wages negotiated in France and the share in Denmark and Germany by using lagged values of our instruments. Another potential threat to identification could be the existence of a technological shock hitting small firms that would be common to France and the country of interest (either Denmark or Germany). Such technological shock would induce a change in the French labor demand by small firms, thus probably changing the wage floors, and would be correlated to both the Danish (for instance) share of workers employed in small firms. We alleviate this concern by controlling for the evolution of the share of workers employed in small firms, which enables to capture labor demand shocks that would affect proportionally more small firms.

Consistently with our model predictions, we find that the share of employees working in small firms has a positive and significant effect on wage floor variations only for unrepresentative industries. This confirms that bargaining firms have differential incentives to raise wage floors whether they are representative or not of the average firm in the industry. The representativeness of employers federations therefore plays a key role in determining collective bargaining outcomes.

This chapter speaks to several strands of the literature. First, it relates to the theoretical literature studying the effect of industry-level agreements on competition (see the seminal

⁴The share of workers employed by small firms for representative industries is constructed as the interaction of the share of workers employed by small firms and a dummy equal to one if the industry is representative

work of Calmfors and Driffill (1988)). The main effects highlighted by the literature are twofold. The closest paper to ours is Haucap, Uwe and Wey (2001), which established that incumbent firms have an interest to raise the industry wage floor in order to impede the entry of firms. Our contribution is to add firms heterogeneity, which enables to distinguish large from small firms interests in the bargaining process, and thus to assess the effect of the domination of employers federations by large firms. Jimeno and Thomas (2013) show that the industry wage floors are a source of wage rigidity as it impedes less productive firms to set a lower wage. We generalize the approach used by these papers by integrating industry-level wage bargaining in a standard firm dynamics model (Melitz (2003)) that allows to account for competition both between heterogeneous monopolistic firms and between industries. Our framework allows for firms entry, which is crucial to analyze the barriers to entry induced by sector-level agreements. Therefore, our model is able to disentangle the different effects highlighted in the previous literature, and shows that they do not offset each other. The cartel effect that we uncover thanks to our model has been confirmed before by the empirical literature. Indeed, Martins (2014) and Magruder (2012) demonstrate that the implementation of industry-level wage floors has a strong negative effect on small firm survival.

The main novelty of the chapter is to emphasize the role of representativeness on collective wage bargaining. To the best of our knowledge, we present the first model focusing on the determination of the employers federations' objective, in presence of firm heterogeneity. We deviate from models with one representative firm, and argue that firms with different productivity levels have different optimal minimum wages.

On the empirical side, Martins and Hijzen (2016) and Hijzen, Martins and Parlevliet (2017) are the only papers underlining the importance of federations' lack of representativeness. More precisely, Martins and Hijzen (2016) explain that "the lack of representativeness of employer associations is a potentially important factor behind the adverse effect of extensions". However, their degree of representativeness, computed as the share of the workforce in affiliated firms to the total employment of the sector, does not increase significantly the effect of extensions. We argue instead that the primary factor is the difference between the interests of decision-makers among employers federations and those of covered firms. More precisely we look at small firms representation, rather than overall representativeness.

This chapter relates naturally to the large literature on the effects of industry-level wage bargaining on unemployment, employment losses and wage rigidities (Díez-Catalán and Villanueva (2015), Dustmann, Fitzenberger, Schönberg and Spitz-Oener (2014), Guriev, Speciale and Tuccio (2016), Hartog, Leuven and Teulings (2002), Martins (2014), Murin, De Serres

and Hijzen (2014), Villanueva (2015)).

This chapter also contributes to a broader debate on the wage inequality effect of unions. Numerous papers analyze the presence of a wage surplus associated with union membership (DiNardo and Lee (2004), Hirsch (2004) or Lewis (1986)). While this may increase the dispersion of wages throughout the economy, collective agreements mechanically raise the wage compression in covered firms. Although these countervailing forces result in an ambiguous theoretical effect, empirical studies have tended to set forth a negative effect of unions on wage inequality (see Frandsen (2012) or Farber, Herbst, Kuziemko and Naidu (2018)).

Finally, this chapter relates to the recent literature exploring the rise of both the labor and product market monopsony. These trends, and the major concerns about its harmful effects, received a large attention from economists (see Van Reenen (2018) for a literature review) and by economic institutions (see CEA (2016)). Indeed, the rise of the product market concentration undermines productive efficiency (see Van Reenen (2011)), raises prices (see De Loecker and Eeckhout (2017)), reduces real wages (see Benmelech, Bergman and Kim (2018)) and increases inequalities (see Hershbein and Macaluso (2018)). Several causes have been presented in order to explain this movement of concentration (see Grullon et al. (2019) who argue that it is generated by a decrease of the anti-trust legislation). Autor et al. (2020) argue that it is generated by superstar firms where more markets become "winners take all". In this article we argue that regulation induces monopsony as it introduces barriers to entry, and allows larger firms to strengthen their dominant position.

The structure of the chapter is as follows. Section 3.2 provides some information about the French institutional setting. Section 3.3 lays out the model and compares industry-level wage bargaining with firm-level bargaining. Section 3.4 characterizes the theoretical impact of representativeness of employer federations. Section 3.5 explores the empirical validity of the model's predictions. Section 3.6 concludes.

3.2 Institutional setting

3.2.1 Collective wage bargaining in France

In almost every OECD country, three distinct levels of minimum wage may coexist: national minimum wage, industry-level minimum wage and firm-level minimum wage. Yet, the predominant level of bargaining starkly differs across countries, as is exhaustively explained in

the OECD Employment Outlook 2017.⁵ In France, the predominant level of wage bargaining is sectoral bargaining: each year around 70% of French total workforce is covered by an industry-level agreement (see Dares (2015b)).⁶

Employers associations and unions negotiate over several topics, among which wages, working time, training, health, severance pay and bonuses. In order to negotiate over wages, the parties must first agree on qualifications levels, and then on a wage floor for each of them. A single industry-level agreement thus often includes several wage floors levels. Table 3.2.1 provides an example of job qualifications and corresponding wage floors for Hairdressing in 2013. In this paper, we ignore the other components of collective bargaining and restrict our attention to wage floor levels as, except for wage floors and qualifications, the negotiation at the firm level enables to opt out from industry-level agreements.⁷

The perimeter of an industry is decided by unions and employers associations and is validated by administrative controls.⁸ Extensions of collective agreements are quasi-automatic in France: once the agreement is signed, except sporadic cases that are not economically significant, the Ministry of Labor extends them to the entire industry, usually within two or three months.⁹ Such quasi-automatic extensions prevail in a large number of European countries, including Italy, Portugal or Spain. The main rationales for such extensions are fairness considerations for workers - *i.e.* to ensure that all workers in a given industry are treated the same way - and transaction costs reduction - *i.e.* to avoid some firms from engaging in lengthy negotiations. However, it has been argued before that extensions could be a tool for “insider firms” to drive competitors out of the market (see Haucap et al. (2001), Magruder (2012), Martins (2014)).

⁵Figure 3.7.1 in the Appendix, taken from the OECD Employment Outlook (2017), provides a summary of the key features of wage bargaining in each OECD country in 2015.

⁶There are around 700 industry-level agreements in France, which cover around 15 millions workers (see Dares (2015b)). Those agreements display a wide heterogeneity in terms of the covered population size. For example, in 2013, 13% of industry agreements covered three quarters of French employees under permanent contracts and 24% covered less than 0.2% of them.

⁷It should also be noted that if firms could not opt out from industry-level agreements regarding those other components, restricting the analysis to wage floor levels in the model would underestimate the effect of collective bargaining.

⁸In a vast majority of cases a firm is covered by a unique industry agreement.

⁹Sources: French Labor Code, OECD Employment Outlook, Fougere, Gautier and Roux (2009). The Ministry may, in principle, exclude from the extension certain clauses of the agreement for legal reasons or reasons of general interest (Labor Code L.2261-25). However, refusals to extend the entire agreement are rare and above all founded on the legal validity of the text - never on economic or social arguments. The possibility for the Ministry of Labor to refuse an extension on a ground of general interest, in particular the objectives of economic and social policy or the protection of the situation of third parties, exists but it is practically never used.

Table 3.2.1 – Grid of qualification and the negotiated wage floors for Hairdressing

CLASSIFICATION	MINIMUM WAGE
LEVEL 1 - GRADE 1 Beginner hairdresser	1484 €
LEVEL 1 - GRADE 2 Hairdresser	1489 €
LEVEL 1 - GRADE 3 Experienced hairdresser	1494 €
LEVEL 2 - GRADE 1 Qualified hairdresser OR Technician	1514 € 1544 €
LEVEL 2 - GRADE 2 Highly qualified hairdresser OR Qualified technician	1635 €
LEVEL 2 - GRADE 3 Very highly qualified hairdresser OR Highly qualified technician	1756 €
LEVEL 3 - GRADE 1 Manager	1911 €
LEVEL 3 - GRADE 2 Experienced manager OR Network leader	2289 € 2701 €
LEVEL 3 - GRADE 3 Highly qualified manager OR Experienced network leader	2863€ 2914 €

3.2.2 The issue of representativeness

Whether extensions are desirable or detrimental is closely intertwined with the representativeness of bargaining institutions - namely the representativeness of both employee unions and employer federations. In France, for an industry agreement to be signed, employees unions have to be representative enough according to a legal threshold which corresponds to 8% of the votes in the last work council elections. In several countries an employer federations representativeness criterion is also applied: for instance, Portugal requires that workers in signing firms represent at least 50% of workers of the industry. In France, no such criterion applied until the 2014 and 2016 laws. Those laws established that an employer federation should represent either 8% of all firms pertaining to employers federations or 8% of all workers of these corresponding firms.

Employer federations representativeness is often defined as “*the share of the workforce in affiliated firms in the total employment of the relevant sector*” (see Martins and Hijzen (2016)). However, a criterion based on this definition does not ensure that signing firms are representative of all the firms covered by the industry agreement. In other words, the representativeness

criterion can be met even if employers federations over-represent large firms, and therefore large firms' interests. Such concern has been expressed in the OECD Employment Outlook 2017: "*Extensions may also have a negative impact when the terms set in the agreement do not account for the economic situation of a majority of firms in the sector. For instance, when the employer association is representative only of large and relatively more productive firms (and hence willing to pay higher wages), it may agree on wage floors and other components that are not sustainable for smaller and less productive firms.*"

A cruel lack of statistics on employer organisations' membership (OECD Employment Outlook 2017) often prevents from providing an adequate picture of employers' federations. Figures on the population covered by collective agreement are usually available: in OECD countries, 26% of small firms workers are covered by a collective agreement while 34% of large firms workers are covered.¹⁰ Yet, information about which firm pertains to which employers federations is largely ignored. Still, it has been documented that large firms are more willing to affiliate than small ones (see Traxler (1995), Traxler (2000), Traxler (1995), Barry and Wilkinson (2011) and Mortimer et al. (2004)). We present in Figure 3.2.1 an index of large firms domination in employers federations, constructed from French data.¹¹ An index higher than one means that the average size of bargaining firms is higher than the average size of all firms in the sector. In other words, for an industry to have a high index means that in this industry, large firms dominate the bargaining process. The histogram exhibits a positively skewed distribution, and most industries display an index superior to one: in most industries, the bargaining firms are the largest firms.

Reasons for this lack of small firms participation can be manifold: lack of time, lack of information, membership contributions. Sociological studies (see Giraud (2012) and Offerlé (2013)) put forward the limited time of small firms' CEOs to fully participate to federations, and therefore negotiations. It is even sometimes argued that some federations refuse small firms as they would not be cost-efficient for the federation: they would contribute low amounts while consuming a lot of the federation's services (see Offerlé (2013)).

¹⁰Source: OECD Employment Outlook 2017

¹¹Public data enables us to know how many firms bargain for each industry-level agreement and how many workers these firms represent. Administrative data (DADS) enables us to know for each industry-level agreement how many firms and workers there are. Merging these two datasets thus enables to compute the average number of workers in bargaining firms and the average number of workers in all firms. The ratio of the two is higher the higher the domination of large firms in industry-level federations.

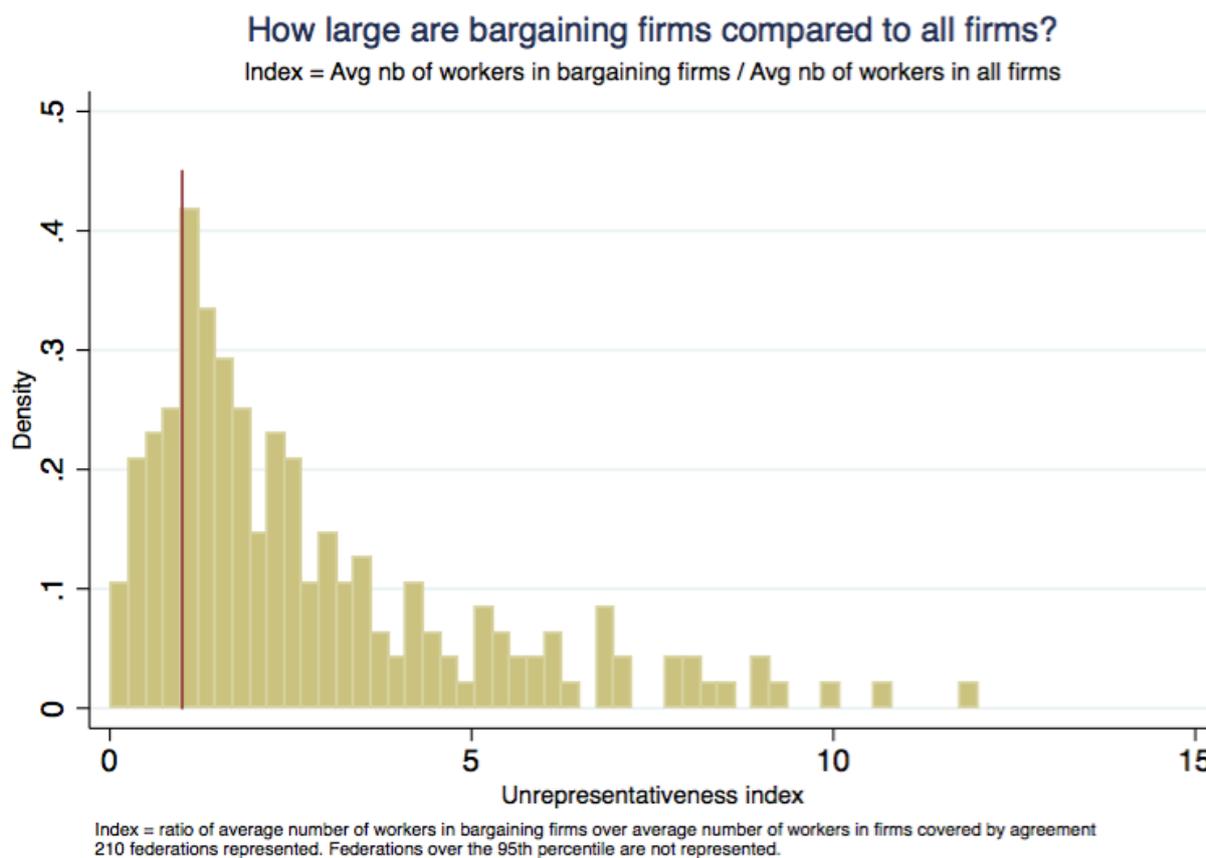


Figure 3.2.1 – Histogram of the index of large firms domination in employers federations

3.3 Model: the impact of industry-level wage bargaining

We use in this section a model very close to Melitz (2003), in which we introduce firm-level bargaining and industry-level bargaining. Going from firm-level to industry-level bargaining raises the productivity threshold, which drives the least productive firms out of the market and which thus benefits the dominant - or most productive - firms. The general equilibrium effects of such sectoral bargaining are an increase in the unemployment rate and a decrease in the utility of consumers. We first lay-out the basic set-up of the model and then study the impact of the bargaining level on the economy.¹²

3.3.1 Setup of the model

Suppose the national market consists of J industries. An industry is composed of a continuum of heterogeneous firms that each produce a single product and operate in monopolistic competition. A representative consumer allocates her consumption between industries on the

¹²We restrict our analysis to a closed economy.

basis of an aggregate industry price, and then between firms of an industry on the basis of the price they charge. We first focus on the demand side, and then we turn to the supply side.

Utility and consumption

The real consumption index of products from industry j is the following Dixit-Stiglitz aggregator:

$$Q_j = \left[\int_{\omega \in \Omega_j} q(\omega)^{\frac{\sigma-1}{\sigma}} d\omega \right]^{\frac{\sigma}{\sigma-1}}$$

where σ is the elasticity of substitution between products of a same industry: $\sigma > 1$ and is constant across industries. $q(\omega)$ is the consumption of product ω , and Ω_j is the set of products of industry j . The representative agent, with total spending E , faces the following maximization program:

$$\max U = \left[\sum_{j=0}^{j=J} \frac{1}{J} (Q_j)^{\frac{\xi-1}{\xi}} \right]^{\frac{\xi}{\xi-1}} \text{ such that } E \geq U.P$$

where $\xi > 1$ is the elasticity of substitution between industries and P is the national aggregate price. Following empirical evidence¹³, we impose $\sigma > \xi$, *ie* we assume that products of the same industry are more substitutable than products of different industries.

We denote $p(\omega)$ the price of product ω and P_j the price index of industry j . Aggregate prices P and P_j write:

$$P = \left[\sum_{j=0}^{j=J} \frac{1}{J} (P_j)^{1-\xi} \right]^{\frac{1}{1-\xi}} \quad (3.1)$$

$$P_j = \left[\int_{\omega \in \Omega_j} p(\omega)^{1-\sigma} d\omega \right]^{\frac{1}{1-\sigma}} \quad (3.2)$$

Following Grossman and Helpman (1991) and Lentz and Mortensen (2008), we choose the numeraire so that the national total spending is equal to E . Then, following Dixit and Stiglitz (1977), for each industry j , aggregate consumption and aggregate revenue equal:

$$Q_j = \frac{1}{J} \left(\frac{P_j}{P} \right)^{-\xi} \frac{E}{P} \quad (3.3)$$

¹³See Lewis and Poilly (2012), Oberfield and Raval (2012), Broda and Weinstein (2006) and Bernard, Eaton and Kortum (2003)

$$R_j = Q_j P_j = \left(\frac{P_j}{P} \right)^{1-\xi} \frac{E}{J} \quad (3.4)$$

The equilibrium price P_j captures the fact that the consumer's utility decreases in the price and increases with the number of products.

Industry composition

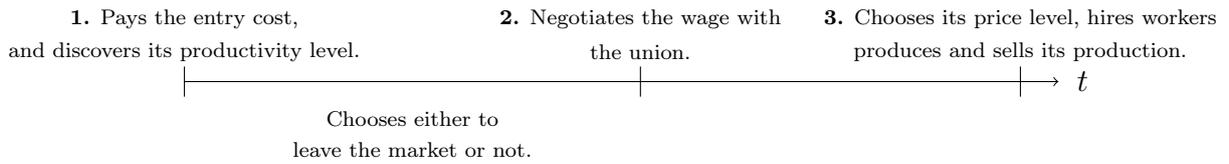


Figure 3.3.1 – Timeline of an entrepreneur.

Production Process Figure 3.3.1 describes the timeline of an entrepreneur. A potential entrant must pay a fixed entry cost f_e to discover its productivity level ϕ , drawn from the distribution $g(\phi)$.¹⁴ Depending on the productivity level drawn, the entrepreneur chooses either to leave the market or not. If the entrepreneur does not exit, she will then in a second step bargain over the wage with a monopolistic union. Then the entrepreneur will hire workers, choose a price and sell her production. We solve the model using backward induction.

Firms' maximization The only factor of production is labor. Within an industry j , an operating firm produces one variety of good ω , faces a fixed cost f , has a productivity level ϕ , charges the price p and pays each worker w . A worker of a firm with productivity ϕ produces ϕ units. Therefore a firm with h workers produces a quantity $q(\omega) = \phi h$ and earns a revenue equal to $r(\omega) = p(\omega)q(\omega)$. The structure of the demand implies:

$$q(\omega) = Q_j \left[\frac{p(\omega)}{P_j} \right]^{-\sigma} \quad (3.5)$$

$$r(\omega) = R_j \left[\frac{p(\omega)}{P_j} \right]^{1-\sigma} \quad (3.6)$$

The firm takes the wage as given when fixing its price.

¹⁴ $G(\phi)$ is the cumulative distribution. This distribution is identical across industries.

Lemma 1 *The price and profits of a firm of productivity ϕ paying a wage w and operating in industry j are given by:*

$$p(\phi, w) = \frac{w}{\rho\phi} \quad (3.7)$$

$$\pi_j(\phi, w) = \frac{R_j}{\sigma} \left(\frac{p(\phi, w)}{P_j} \right)^{1-\sigma} - f \quad (3.8)$$

where $\rho = \frac{\sigma-1}{\sigma}$ is the profit-maximizing markup set by the firm.

Firms' entry and equilibrium structure of an industry The equilibrium structure of an industry is characterized by a mass of firms M_j that enter the market¹⁵ and a distribution of productivity levels, which are both pinned down by the zero cutoff profit condition and the free entry condition.

The zero cutoff profit condition - or equivalently firms destruction condition - states that a firm produces if and only if its profits are positive. This condition implies the following relationship between the average profit $\bar{\pi}_j$ in the industry and the productivity cutoff ϕ_j^* , which is the minimum productivity level generating a non-negative profit:

$$\pi(\phi_j^*) = 0 \Leftrightarrow \bar{\pi}_j = \frac{1}{1 - G(\phi_j^*)} \int_{\phi_j^*}^{\infty} \pi_j(\phi)g(\phi)d\phi = f [\Gamma(\phi_j^*) - 1] \quad (3.9)$$

where

$$\Gamma(\phi^*) = \frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \left[\frac{w(\phi)\phi^*}{w(\phi^*)\phi} \right]^{1-\sigma} g(\phi)d\phi$$

The free entry condition - or equivalently firms creation condition - indicates that a prospective firm pays the entry cost only if the expected profit from entry - equal to the average profit conditional on surviving, multiplied by the probability of surviving - is higher than the entry cost. Therefore, the free entry condition defines a positive relationship between the average revenue $\bar{\pi}_j$ and the productivity threshold ϕ_j^* :

$$\bar{\pi}_j = \frac{f_e}{1 - G(\phi_j^*)} \quad (3.10)$$

The zero cutoff profit and free entry conditions will enable to pin down the production cutoff ϕ_j^* and the average profit $\bar{\pi}_j$.¹⁶

¹⁵Entering firms are firms choosing to pay the entry cost.

¹⁶This will be proved in Proposition 3.3.1. Proposition 3.3.1 will demonstrate that both the profits and

The value of the mass of firms is given by the aggregate industry demand.

Aggregate variables Based on previous results, we derive the formulas for the aggregate price index, P_j , the aggregate profit, Π_j , the aggregate revenue, R_j , and the aggregate employment, L_j , given in Appendix 3.7.2.

3.3.2 Impact of the introduction of industry-level bargaining

In this section, we study the impact of the implementation of industry-level bargaining - ie bargaining system where an industry-wide union and an industry-wide employers federation bargain over a binding industry's minimum wage - relative to the baseline situation of firm-level bargaining. We use in the following the subscript f and i for respectively the firm-level bargaining and the industry-level bargaining.

When wages are negotiated at the industry level, firms internalize that a rise in the wage floor will increase the competitors' costs. Thus, the introduction of industry-level bargaining raises wages. Moreover, we find that sector-level agreements impede the least productive firms from producing and increasing the profits of the largest firms. The general equilibrium effects of the industry-level bargaining are a decrease of both employment and consumers utility. In both bargaining systems we use a right-to-manage model (see Nickell and Andrews (1983)).¹⁷

Firm-level bargaining

After paying the entry cost and having discovered its productivity level, an entrepreneur bargains over the wage with a unique union. In the firm-level bargaining situation, the union represents all workers of the economy and maximizes the sum of their expected utilities minus their reservation utilities.¹⁸ Following the literature (see Cahuc et al. (2014) for a summary), we assume that there is a reservation wage denoted by \tilde{w} , which is an exogenous parameter

the wage are increasing in the productivity level ϕ . Consequently, equation 3.9 defines a negative relationship between the production cutoff ϕ_j^* and the average profit $\bar{\pi}_j$. Therefore, the zero cutoff profit and free entry conditions define equilibrium values of ϕ^* and $\pi_{j(\phi^*)}$. The determination of the equilibrium is depicted in Figure 3.3.2.

¹⁷In such a model the firm and the union negotiate only over the wages, ie the firm still chooses its employment level. This assumption is made in order to maximize comparability between scenarios. Indeed, it would be highly counter-factual to assume that, at the industry-level, unions and employers federations bargain over the level of employment.

¹⁸The utility of the union when bargaining occurs with firm f is : $\nu_f = l_f u(w) + (L - l_f)\tilde{u}$, where \tilde{u} is the reservation utility of individuals and L the population size. In our setting, $u(w) = w$ and $\tilde{u} = \tilde{w}$. The utility of the union in case of bargaining failure is $\tilde{\nu} = L\tilde{u}$. Therefore the union maximizes $\nu_f - \tilde{\nu} = l_f(u(w) - \tilde{u}) = l_f(w - \tilde{w})$.

representing an unemployed person's utility. We also assume, for simplicity and at no cost for the results, that the utility of a worker is directly given by its wage. Finally, we denote $l_f(\phi, w)$ the employment level of firm f .

Lemma 2 *When the negotiation takes place at the firm level, the wage paid by a firm of productivity ϕ is given by*

$$\max_w \left\{ [l_f(\phi, w)]^\beta [w - \tilde{w}]^\beta \left[p_f(\phi, w) q_f(\phi, w) - w \frac{q_f(\phi, w)}{\phi} - f \right]^{1-\beta} \right\} \quad (3.11)$$

Where β is the bargaining power of unions, considered as being identical across firms. This directly implies the following proposition

Proposition 3.3.1 *When the negotiation takes place at the firm level, wages and profits fulfill the following propositions (Proof are given in Appendix 3.7.3)*

A The wage is an increasing function of productivity and fulfills the following condition

$$\tilde{w} \leq w_f(\phi) \leq w_f^{max} = \left[1 + \frac{\beta}{\sigma - 1} \right] \tilde{w} \quad (3.12)$$

B Profits are increasing in the productivity level ϕ .

A first important implication of Proposition 3.3.1 is the existence and unicity of the productivity threshold ϕ_j^* . Because the wage is perfectly pinned down by the productivity level and because there is a positive relation between profits and productivity, equation 3.9, derived from the zero cutoff profit condition, defines a negative relationship between the average revenue $\bar{\pi}_j$ and the productivity threshold ϕ_j^* . Together with the free entry condition defined in equation 3.10, this proves that the productivity threshold exists and is unique. The determination of the equilibrium is depicted in Figure 3.3.2a in the $(\phi, \bar{\pi})$ space.

A key result of the model is that more productive firms pay higher wages. Indeed, because more productive firms have higher profits and the wage elasticity of revenue is invariant with productivity, the absolute value of the wage elasticity of profit is lower in the most productive firms.

Firms with a productivity level equal to the productivity cutoff pay a wage equal to the reservation wage - ie $w_f(\phi^*) = \tilde{w}$. Those marginal firms are indifferent between producing or

not. Similarly workers of those firms are indifferent between being employed or not. When productivity tends to infinity the wage converges toward its upper bound. This maximum wage increases with the outside option and with the bargaining power of the union. Furthermore, it decreases with the substitutability of competitors products: the higher the value of σ , the more sensitive profits are to a wage increase.

Industry-level bargaining

In this section we derive the equilibrium structure of the industry when employers and workers negotiate a binding minimal wage which applies to the entire industry. A single union represents all the workers in the economy.¹⁹ The employers federation represents firms which choose to produce - *ie* firms that exist at step 2 of Figure 3.3.1.

In the following, we first describe the impact of the introduction of a binding minimum wage on the structure of the industry. Then we turn to the analysis of the bargaining game and its impact on the equilibrium structure of the industry.

Features of the industry's minimum wage In the industry-level bargaining scenario, an industry-wide employers federation and an industry-wide union negotiate a wage floor. To account for institutional features of countries using industry agreements, we assume that the final wage floor is the maximum of the industry-level and the firm-level bargained wage floors.²⁰ In other words, the industry wage floor is only binding for firms that would have negotiated, at the firm level, a lower wage.²¹

Impact of the industry's minimum wage on the equilibrium structure of the industry We denote the industry's minimum wage \bar{w} . Introducing industry-level wage bargaining shifts the zero cutoff profit condition, which will then impact the equilibrium structure of the industry. Results are summarized in the following Proposition.

Proposition 3.3.2 *When an industry's minimum wage is implemented the equilibrium structure of the industry fulfills the following propositions (Proof are given in Appendix 3.7.4)*

¹⁹The utility of the union when bargaining occurs for industry i is : $\nu_i = l_i u(w) + (L - l_i)\tilde{u}$, where \tilde{u} is the reservation utility of individuals and L the population size. In our setting, $u(w) = w$ and $\tilde{u} = \tilde{w}$. The utility of the union in case of bargaining failure is $\tilde{\nu} = L\tilde{u}$. Therefore the union maximizes $\nu_i - \tilde{\nu} = l_i(u(w) - \tilde{u}) = l_i(w - \tilde{w})$.

²⁰In every country with sector-level bargaining, workers can be, and generally are, paid more than the wage floor (see Gautier, Fougère and Roux (2016) for France).

²¹In industries using wage floors, we still observe firm-level agreements (see Avouyi-Dovi, Fougère and Gautier (2013) for France).

A As long as the industry wage floor is between the reservation wage \tilde{w} and w_f^{max} , both the average revenue and the productivity cutoff increase with the wage floor.

B As long as the industry's minimum wage is not binding for all firms²², the higher it is the higher firms' profits. Equivalently: $w_i(\phi, \bar{w}) > \bar{w} \Rightarrow \frac{\partial r_i(\phi, \bar{w})}{\partial \bar{w}} > 0$.

The higher the industry's wage floor, the higher the productivity cutoff, thus the lower the number of firms in the market. Furthermore, among producing firms, the wage floor will increase the labor cost of firms for which it is binding. Those two mechanisms will decrease competitiveness of the industry and, consequently, will increase the profits of the largest firms.

Figure 3.3.2 depicts the equilibrium structure of the economy under firm-level versus industry-level bargaining. Figure 3.3.2a represents the equilibrium profits under each wage bargaining system. The implementation of an industry-level wage floor shifts the zero cutoff production curve to the right, while it has no impact on the free entry condition. Consequently, under industry-level bargaining, both the equilibrium productivity cutoff and the equilibrium value of profit increase. Figure 3.3.2b represents the equilibrium distribution of profits. The wage floor increases the prices set by the least productive firms, which decreases their profits and, as a consequence, it increases the profit of the largest firms.

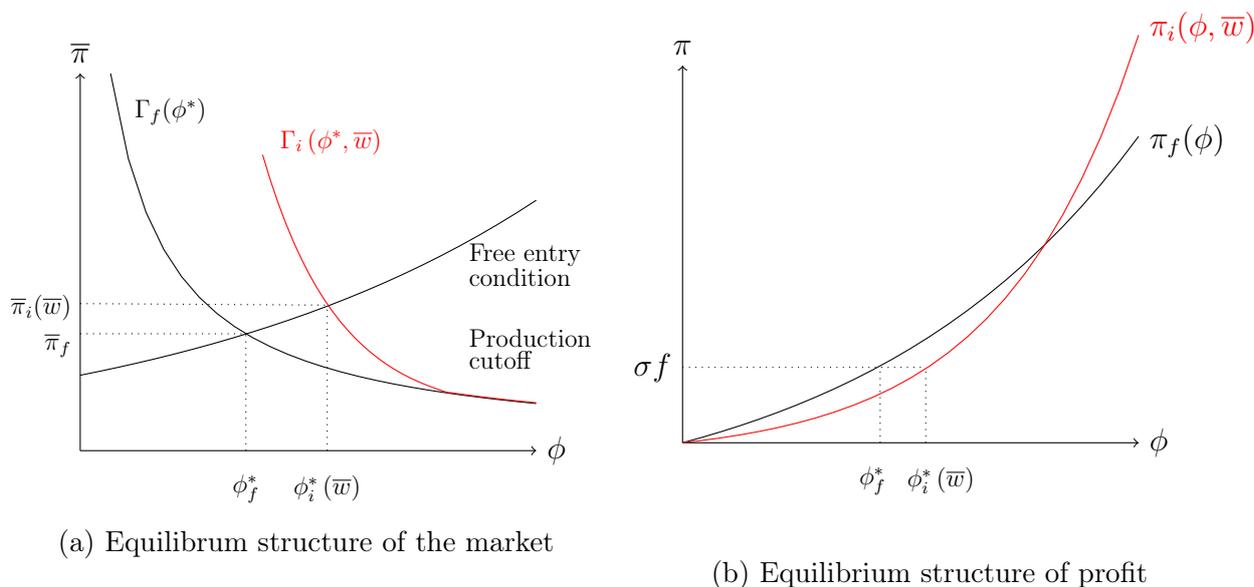


Figure 3.3.2 – Impact of industry level bargaining on industry structure

Note: Black curves represent the situation under firm-level bargaining, red curves under industry-level bargaining. $\bar{\pi}_f$ and $\bar{\pi}_i$ respectively denote the average profit in the industry under firm-level bargaining and under industry-level bargaining, ϕ_f^* and ϕ_i^* the productivity cutoff under firm-level bargaining and under industry-level bargaining, \bar{w} the industry wage floor, ϕ the productivity level.

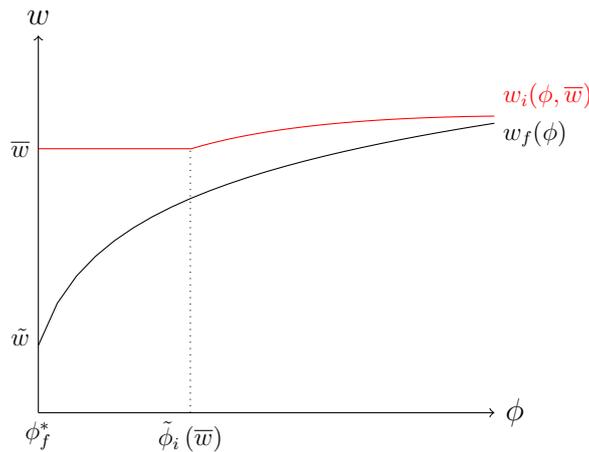


Figure 3.3.3 – Impact of wage floor on wages

Note: The black and red curves respectively represent the relationship between the wage w and the firm's productivity ϕ under firm-level bargaining and under industry-level bargaining. ϕ_f^* denotes the productivity cutoff under firm-level bargaining. $\tilde{\phi}_i(\bar{w})$ represents the higher level of productivity such that under industry-level bargaining a firm pay a wage equal to the wage floor

Figure 3.3.3 depicts the wages both under firm-level bargaining and under industry-level bargaining²³. First, firms of low productivity negotiate firm-level wages lower than the industry wage floor, consequently compliance with the industry's minimum wage implies that they pay a wage equal to the wage floor. In the figure, we define a productivity level $\tilde{\phi}_i(\bar{w})$ which represents the higher level of productivity such that firms pay a wage equal to the wage floor. However, industry-level wage floors raise wages even for firms for which they are not binding - *ie* firms for which $\phi > \tilde{\phi}_i(\bar{w})$. There is indeed an indirect source of wages increase: industry-level bargaining raises large firms' profits and, because the wage negotiated at the firm-level increases with the level of profits, wages paid by those firms are higher than without industry-level wage bargaining.

The wage bargaining process At the industry level an employers federation, representing the interests of producing firms, negotiates with the employees union. We suppose that, for the employers association, each firm has the same weight in the aggregate industry's objective. Therefore, the aggregate profit is the sum, over all firms, of the employers' profits. The employees union's objective is now evaluated at the industry level. Consequently, the objectives are given by:

Union's objective :

²²Ie when $\bar{w} < w_f^{max}$

²³We represent the industry-level bargaining situation for which there is a wage floor fulfilling the condition $\bar{w} < w_f^{max}$

$$U_i(\bar{w}) = \left[\frac{1}{1 - G(\phi_i^*)} \int_{\phi_i^*}^{\infty} (w_i(\phi, \bar{w}) - \tilde{w}) l_i(\phi, \bar{w}) g(\phi) M d\phi \right] \quad (3.13)$$

Employers federation's objective :

$$V_i(\bar{w}) = \Pi_i(\bar{w}) = \frac{1}{1 - G(\phi_i^*)} \int_{\phi_i^*}^{\infty} \pi_i(\phi, \bar{w}) M g(\phi) d\phi \quad (3.14)$$

Lemma 3 *When an industry wage floor is negotiated, it solves the following problem:*

$$\max_{\bar{w}} \left\{ \left[\frac{1}{1 - G(\phi_i^*)} \int_{\phi_i^*}^{\infty} (w_i(\phi, \bar{w}) - \tilde{w}) l_i(\phi, \bar{w}) g(\phi) M d\phi \right]^{\beta} [\Pi_i(\bar{w})]^{1-\beta} \right\} \quad (3.15)$$

Proposition 3.3.3 *The industry wage floor satisfies the following propositions (Proofs are given in Appendix 3.7.4)*

A The wage floor is finite, is strictly higher than the reservation wage and has an upper bound denoted \bar{w}^{max} .²⁴

$$\tilde{w} < \bar{w} \leq \bar{w}^{max} = \tilde{w} \left[1 + \frac{\beta}{\xi - 1} \right]$$

B The introduction of industry-level wage bargaining increases the productivity cutoff, the average profit and the wage paid by every firm.

The intuition behind Proposition 3.3.3.A is the following. The most productive firms benefit from an increase in the wage floor as it raises their profits. For firms for which the wage floor is binding though, profits decrease with the wage floor. However such a negative impact on profits is partly offset by the increase in competitors labor cost. Therefore, overall firms have an incentive to raise the negotiated wage above the reservation wage.

Yet, as products of different industries are substitutable, the most productive firms do not capture the entire loss of revenue of the least productive firms in their industry. Indeed, a raise in an industry's wage floor will compel the representative consumer to allocate a lower

²⁴The upper bound is the wage that would be paid by a firm with revenue equal to \bar{r}^{max} , facing competition with enterprises manufacturing products having an elasticity of substitution equal to ξ .

share of the national aggregate revenue in goods of this industry. Therefore, the industry's aggregate revenue will decrease after an increase of the wage floor, ensuring that there is a finite solution. Indeed, the industry's minimum wage is bounded from above because when it becomes binding for all firms it does not generate any relative labor cost increase. The negotiating parties then only account for the competition with other industries.

Because the upper bound of the industry-level wage floor is superior to the upper bound of firm-level negotiated wages - ie $\bar{w}^{max} > w_f^{max}$ -, a situation in which the industry-level wage floor is binding for every firm of the industry can arise. In other words, it is possible that $\bar{w} \in [w_f^{max}, \bar{w}^{max}]$ ²⁵, but this seems like a very unrealistic scenario.

It must be noticed that previous results crucially rely on two assumptions. First, an entrepreneur cannot opt-out from an industry-level agreement. Second, we consider that every firm is covered by the agreement, and not just those that are members of the employers federation. This assumption is made to account for institutional features of countries using industry-level agreements. For example, in France, the ministry of Labor extends the agreements quasi-automatically beyond the limits of signing parties. If we relax this assumption, this would generate an equilibrium equivalent to the one with only firm-level agreements. Indeed, if the wage floor was binding for an entrepreneur she would choose to freely exit the employer organization and to implement the wage negotiated at the firm level.

General equilibrium

In this section we derive the structure of a symmetric equilibrium, in which the wage is negotiated at the same level in every industry. We find that industry-level bargaining raises unemployment and decreases the utility of the representative consumer.

We focus on the national aggregate spending and assume that there are no savings so firms' profits and wages are entirely consumed. The fixed cost for operating is paid to other firms in the economy, thus it increases the aggregate demand. The entering cost f_e investment cost is financed by a loan, that has to be paid off later, and we do not consider equilibrium situations where there is either national savings or national deficit. Then, at the equilibrium, the impact of investment on profits of operating firms and on demand sum to zero.

²⁵The closer the elasticities of substitution across and within industries ξ and σ , the lower the probability of such a case to arise.

Lemma 4 *The aggregate resource in the economy is given by*

$$E = \left\{ \sum_{j=1}^{j=J} \frac{1}{1 - G(\phi_k^*)} \int_{\phi_k^*}^{\infty} \left[\pi_k(\phi) + w_k(\phi) \frac{q_k(\phi)}{\phi} + f \right] g(\phi) M_k d\phi \right\}_{k \in \{f,i\}} \quad (3.16)$$

Definition 1 *A steady-state equilibrium is a set of productivity cutoffs and average revenues $\{\phi_k^*, \bar{r}_k\}_{k \in \{f,i\}}$, wages $\{w_k(\phi)\}_{k \in \{f,i\}}$, prices $\{p_k(\phi)\}_{k \in \{f,i\}}$ and masses of firms $\{M_k\}_{k \in \{f,i\}}$ such that*

1. *The productivity cutoff and the average revenue solve both the free entry condition and the zero cutoff profit condition.*
2. *The wage solves bargaining equation 3.15 under industry-level bargaining and equation 3.11 under firm-level bargaining.*
3. *Price settled by firms always solve equation 3.7.*
4. *The aggregate revenue solves equation 3.4.*
5. *Price level fulfills the condition on the numeraire.*
6. *The aggregate resource constraint, equation 3.16, is satisfied.*

Impact of level at which the bargaining takes place The first three conditions ensure that the productivity cutoff is higher when a wage floor is implemented, as developed above. Using the following propositions it can be derived that

Proposition 3.3.4 *Introducing industry-level bargaining raises unemployment and decreases consumers' utility (Proof given in Appendix 3.7.5).*

As wages are higher when there is an industry wage floor, hired workers capture a higher share of firms' revenues. Therefore, this will reduce the labour demand, and so employment. Moreover an industry's minimum wage will decrease the variety of the economy, which decreases the utility of consumers. Finally, as wage floors increase the labor cost, it will increase prices of goods, which also has a negative impact on the utility.

3.4 Model: the role of the representativeness of bargaining institutions

In section 3.3 we demonstrate that the implementation of industry-level bargaining leads to higher wages, thus conducing to the eviction of small firms. In this section we study the impact of the over-representation of large firms' interests in employers federations. Large firms' profits increase when the wage floor increases, consequently the more the objective of the employers federation takes into account the interests of large firms, the higher the negotiated wage floor. As a result, the productivity cutoff increases and the variety of the industry, the employment and the utility of the consumers decrease.

Similarly to the previous section, we do not endogenize the choice between the firm-level bargaining scenario and the sector-level bargaining scenario. However, this would strengthen our results. Indeed large firms always have an interest to implement a wage floor. Consequently, when their domination increases, they will choose to implement it.

3.4.1 Representativeness of employers federations

This subsection focuses on the equilibrium structure of the economy when the employers federation over-represents the interests of the largest firms during the industry-level bargaining process. We denote h the type of such a bargaining institution. We provide in Appendix 3.7.6 a micro-foundation explaining the existence of such bargaining institution - ie an institution that attributes higher weights to larger firms

As firms are heterogeneous in terms of productivity, their objectives may differ. Yet, when negotiating at the industry level, the employers organization must aggregate them into a single objective. Previously, we assumed that the employer association gives the same importance to each firm, but this appears to be far from reality. Indeed, large firms are over-represented in the composition of employers federations (see, for France, Dares (2015a)). Furthermore, the domination of large firms in employers organizations has been well established by the literature (see Traxler (2000) for example).

In order to take into account this phenomenon we assume that, at the industry level, the aggregate objective of the employers organization is built on a voting system where the larger the firm the larger its share of votes. In this section, the voting share of a firm depends

only on its productivity, and is denoted $h(\phi)$. The definition of an employers federation that over-represents interests of large firms is given by

Definition 2 *An unrepresentative employers federation is defined by an objective $V_h(\bar{w})$ and a function $h(\cdot)$ belonging to the set of functions H , where :*

$$V_h(\bar{w}) = \frac{\int_{\phi^*(\bar{w})}^{\infty} \pi(\bar{w}, \phi) g(\phi) h(\phi) M d\phi}{\int_{\phi^*(\bar{w})}^{\infty} g(\phi) h(\phi) d\phi}$$

$$h \in H \Leftrightarrow \left\{ \forall x \in [0, \infty] h'(x) \geq 0; \int_0^{\infty} h'(x) dx > 0 \text{ and } \int_0^{\infty} h(x) g(x) dx = 1 \right\}$$

The first condition on H implies that the voting share is weakly increasing in productivity. The second one ensures that it differs from the "equal votes" case, and the last condition implies that $h(\cdot)$ represents weights. We define $g_h(\cdot) = g(\cdot)h(\cdot)$, the last condition thus implies that $g_h(\cdot)$ can be understood as being a distribution of firms, constructed from the real distribution of firms ($g(\cdot)$), and from their vote share ($h(\cdot)$). Finally, we define $G_h(\cdot)$ as being the cumulative distribution of the previous function. The aggregate employers federation's objective becomes

$$V_h(\bar{w}) = \frac{1}{1 - G_h(\phi^*(\bar{w}))} \int_{\phi^*(\bar{w})}^{\infty} \pi_h(\phi, \bar{w}) M g_h(\phi) d\phi \quad (3.17)$$

During the bargaining process, on the employer side, everything happens as if it was an "equal votes" system but with a more positively skewed distribution of firms. Definition 3 characterizes a ranking among the set H of functions on the basis of the magnitude of large firms over-representation.

Definition 3 $\tilde{h}(\cdot)$ represents an economy in which large firms interests are more valued than the economy represented by $h(\cdot)$ when :

$$\left\{ \left\{ \tilde{h}(\cdot), h(\cdot) \right\} \in H^2; \forall x \in [0, \infty] (\tilde{h}'(x) - h'(x)) \geq 0; \int_0^{\infty} (\tilde{h}(x) - h'(x)) dx > 0 \right\}$$

The bargaining problem is presented in Lemma 5: the only difference with the section 3.3 is that the employers objective is now $V_h(\bar{w})$ instead of $V_i(\bar{w})$ (defined in equation 3.14).

Lemma 5 *When the negotiation takes place at the industry level and when interests of the largest firms are over-represented, the bargaining problem is given by*

$$\max_{\bar{w}} \left\{ \left[\frac{1}{1 - G(\phi^*(\bar{w}))} \int_{\phi^*}^{\infty} (w(\phi, \bar{w}) - \tilde{w}) l_f(\phi, \bar{w}) Mg(\phi) d\phi \right]^{\beta} [V_h(\bar{w})]^{1-\beta} \right\} \quad (3.18)$$

When large firms are over-represented in an employers federation - *ie* when the federation is unrepresentative, the negative impact of a minimum wage increase for the federation becomes less important. Indeed, this negative effect is concentrated among the smallest firms, and their interests are less accounted for. Moreover, large firms have an interest to raise the wage floor, which is more reflected in the industry's aggregate objective. Those two effects will have a positive impact on the negotiated wage.

The over-representation of large firms will also increase the negative impact of the industry's wage floor on the variety of the industry as the wage floor increase will impede more firms to produce. Therefore, we can derive the following proposition.

Proposition 3.4.1 *When representativeness of labor market institutions changes, as long as the wage floor is not binding for every operating firm, the economy fulfills the following propositions (Proofs are given in Appendix 3.7.6)*

- A The higher the over-representation of large firms interests, the higher the wage floor and the wage paid by every operating firm.*
- B The higher the over-representation of large firms, the higher the average revenue and the productivity cutoff.*

3.4.2 General equilibrium

The general equilibrium fulfills equations given in definition 1. Then, using the results of Proposition 3.4.1 we can derive the following Proposition.

Proposition 3.4.2 *As long as the minimum wage is not binding for every operating firm, the more interests of large firms are over-represented, the higher the unemployment rate and the lower the consumers' utility.*

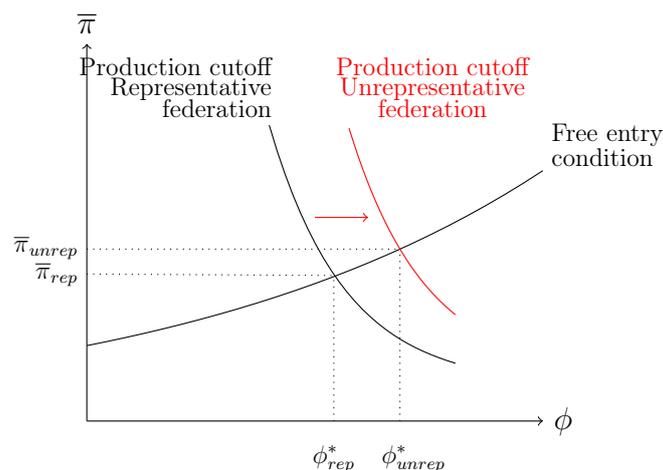


Figure 3.4.1 – Equilibrium structure of the market under representative and un-representative industry-level bargaining

Note: The dark and red curves respectively represent the zero cutoff production (ZCP) condition under industry-level bargaining with representative and unrepresentative employers federations. ϕ_{rep}^* and ϕ_{unrep}^* respectively denote the productivity cutoff with a representative and unrepresentative employers federation, π_{rep} and π_{unrep} respectively denote the average profit in the industry with a representative and unrepresentative employers federation. The more unrepresentative the employers federations, the more the ZCP curve is shifted to the right, thus increasing the productivity cutoff ϕ^* .

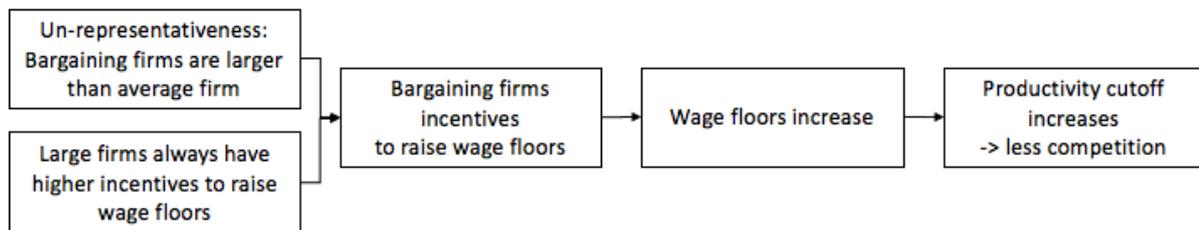
As the over-representation of large firms interests leads to a wage floor increase, the negative effects of the baseline "equal weights" system will be strengthened. Therefore, this will reduce the variety of products and increase prices, reducing consumers' utility. This implies that the representativeness of negotiating institutions has a strong impact on the negotiation outcomes, and thus the equilibrium structure.

3.5 Empirical evidence on the impact of employers federations unrepresentativeness

In this section we explore the empirical validity of the model's predictions, namely the positive relationships between the over-representation of large firms' interests in employers federations and both bargained wage floors and product market concentration. In a first step we document novel stylized facts regarding the positive correlation between federations unrepresentativeness and product market concentration. The theoretical mechanism of our model which explains such a positive correlation is that bargaining firms have higher incentives to raise wage floors when they are larger than the average firm of the industry - *i.e.* in unrepresentative federations. In a second step, we provide causal evidence corroborating this mechanism thanks to novel data about employers federations representativeness in France.

3.5.1 Testing the model's predictions

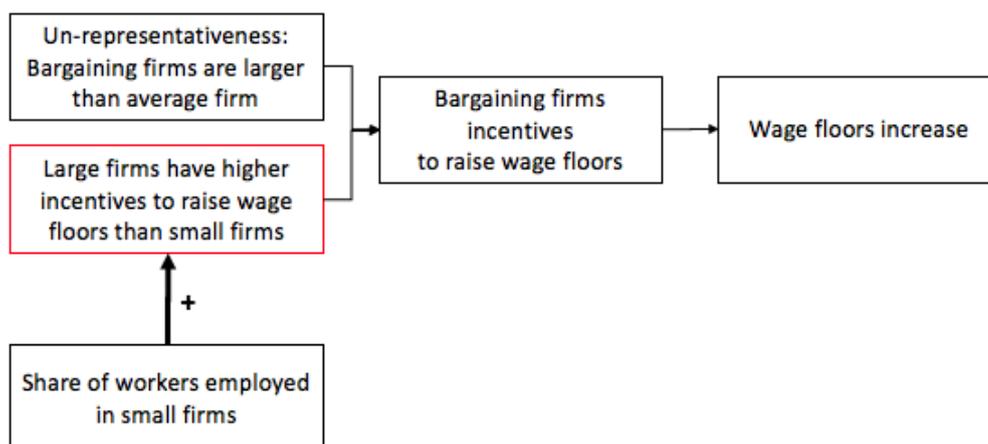
The first part of our model establishes that large firms always have higher incentives than small firms to raise the wage floors because it enables them to evict the small firms from the market. However, for that to translate into higher wage floors, bargaining firms must be the large firms. Therefore, the over-representation of large firms in employers federations - that we call unrepresentativeness of federations - is a crucial component to understand the outcomes of the bargaining system. These mechanisms are depicted in Figure 3.5.1 below.



Note: All the mechanisms depicted above are results of our model. One result of our model is that large firms have higher incentives to raise wage floors. The higher the unrepresentativeness of employers federations, the higher the incentives of bargaining firms to raise wage floors.

Figure 3.5.1 – Results from our theoretical model

The main mechanism highlighted in our model is therefore that the higher the over-representation of large firms interests in employers federations, the higher the bargaining firms incentives to raise the wage floor, and thus the higher the wage floor. In other words, bargaining firms have differential incentives to raise wage floors whether they are representative or not of the average firm in the industry. However, this mechanism cannot be directly tested because bargaining firms incentives to raise the wage floor are unobservable by nature.



Note: Because large firms' incentives to raise the wage floors are by nature unobserved, we use the share of workers employed in small firms as a proxy. The higher the share of workers employed in small firms, the higher the competition faced by large firms, and therefore the higher their incentives to evict the small firms from the market.

Figure 3.5.2 – Testing the model's predictions

We solve this problem by using a variable shifting the large firms' incentives to raise wage

floors: the share of workers employed by small firms. The higher the share of workers employed by small firms, the higher the incentives for large firms to increase bargained wage floors. Indeed, the higher this share, the higher the competition from small firms, and thus the higher the large firms' incentives to evict small firms from the market. If bargaining firms are the largest firms - *i.e.* in unrepresentative industries, then the share of workers employed in small firms should have a positive effect on the bargained wage floors. On the opposite, in representative industries, the share of workers employed by small firms should not have any effect on the bargained wage floors. We build an index capturing the representativeness of the employers federations bargaining at the sector level and estimate the effect of the share of workers employed by small firms on wage floors for representative and unrepresentative industries.

Because it is highly likely that industries diverge in their wage floors due to some unobserved industry-level factors, such as productivity or unions history, we conduct the analysis *within* industry. We indeed use the fact that within a single industry agreement, several skill-dependent wage floors coexist to compare wage floors within a given industry. This enables us to alleviate several of the most obvious endogeneity concerns by removing any unobserved industry-level heterogeneity.

3.5.2 Data

The empirical analysis draws on three French datasets. The novelty of our approach lies in particular in the merge of wage floor data with employers federations' representativeness indices.

Matched employer-employee data We first use French matched employer-employee data - DADS Postes - containing information on every French employee over the period 2008-2014 (*Déclarations annuelles de données sociales*). This dataset contains a vector of information on each employee (size of the firm he operates in, annual earnings, annual hours worked, industry he operates in etc.) and includes the administrative number of the industry-level agreement covering the employee.

Wage floor data The second data set contains around 48,000 wage floors established between 2008 and 2014, split between the 345 largest industry agreements in terms of number of covered employees. All the data can be freely collected on a French governmental website.²⁶

²⁶<http://www.legifrance.gouv.fr/initRechConvColl.do>

This data set contains the name of the industry agreement, its administrative number, the name of unions signing it, the date of the agreement, the date of the enforcement and the negotiated wage floors. Within a given agreement several skill-level wage floors are bargained over. We define a wage floor level as the increasing order of the wage floor within the industry agreement - a higher level meaning both a higher worker qualification and a higher wage floor. Table 3.2.1 provides an example of job qualifications and corresponding wage floors for Hairdressing in 2013.

Using the number of the industry-level agreement, we merge the two aforementioned datasets - namely DADS Postes and wage floor data. Because in the matched employer-employee dataset wages appear at the annual level, we annualize bargained wage floors.²⁷ A caveat of DADS Postes is that, though the industry agreement of each worker is known, the wage floor skill-level is not, thus preventing us from perfectly matching each worker to her adequate wage floor level \times industry agreement. To merge the two data sets, we instead make the assumption that the modes of the distribution of the base wages correspond to those of the contractual wages set by collective bargaining (see Cardoso and Portugal (2005) for an empirical justification). Merging the datasets enables us, among other, to compute, for each wage floor, the features of the population of covered workers. We use the 2-digit French classification of socio-professional categories (*PCS-ESE*), which contains 42 occupations, to identify the most frequent occupation of each wage floor.

Domination of employers federations by large firms In 2017, the results of the employers federations elections were made public by the DARES (*Direction de l'Animation de la Recherche, des Etudes et des Statistiques*) following the March 2014 French law on federations representativeness. 2017 is therefore the first year the representativeness of employers federations is measured in France. The representativeness criterion set by the 2014 law is that each federation represent at least 8% of the total number of firms in the industry affiliated to a federation.²⁸ It must be noted that this representativeness criterion does not take into account the percentage of affiliated firms: a federation can be called "representative" even if only a tiny portion of firms are affiliated. Rather than this criterion, we use in our subsequent analysis some statistics on affiliated firms, in particular the average size of bargaining firms as compared to the size distribution within the industry.

Sample restriction The final sample covers the 2008-2014 period. We restrict the analysis

²⁷In order to annualize wage floors, we simply compute the annual value of a wage floor by averaging its value over the year.

²⁸Or that those firms represent at least 8% of workers

to full time workers covered by a wage agreement, aged between 18 and 60 years old and working in firms with more than 9 workers.

German firm-level data set We have access to a data set that contains all German firms, their size, the industry in which they are operating (5 digit of the NACE classification), over the period 2005-2014 (Establishment History Panel BHP produced by the IAB).

Danish firm-level data set We have access to a data set that contains all Danish firms, their size, the industry in which they are operating (5 digit of the NACE classification), over the period 2005-2014 (FIRM data base).

3.5.3 Indices construction and descriptive statistics

In this sub-section we construct two indices of interest for testing the model's predictions, namely the share of workers operating in small firms and an index of federations' representativeness.

Share of workers operating in small firms We calculate, for each wage floor i , time t and industry-level agreement j , an index equal to the share of employees operating in small firms as follows:

$$S_{ijt} = \frac{\sum_{k \in J} \mathbb{1}(pos_k = i) \mathbb{1}(N_{kt} \leq 50)}{\sum_{k \in J} \mathbb{1}(pos_k = i)} \quad (3.19)$$

where J denotes the set of workers operating in industry j , k refers to a worker, N_{kt} is the size of the firm he is operating in, pos_k is the wage floor covering the individual k . Constructing an index at the wage-floor level will allow us to exploit within-industry and year heterogeneity in the subsequent estimations. In line with our model's predictions, the higher this index, the larger the interests of large firms to increase wage floors. We should therefore observe empirically a positive impact of this index on wage floors variations.

Index of federations domination by large firms In 2017, DARES published results of employers federations elections. Such results enable to reconstruct for each industry agreement j the average size of the bargaining firms. Comparing this size to the average size of all firms of the industry - *i.e.* bargaining and non-bargaining firms - enables to compute an index of domination of federations by large firms. Formally: for each industry j we treat bargaining firms as one representative firm of size L_j and compute the percentile of firm size

distribution of the industry corresponding to size L_j .²⁹ The higher the percentile, the larger the bargaining firms as compared to the other firms of the industry. In the following, we will interchangeably denote this measure the 'unrepresentativeness measure' or the 'measure of large firms domination of federations'.

The index is computed in 2017 and is used to measure the representativeness of employers federations over several years. However, it has been documented that there is an important persistence among employers federations (see Mahoney and Thelen (2010)). On this basis we argue that, if employers federations are not representative in 2017, they were not on the entire period studied.

Descriptive statistics Table 3.5.1 summarizes statistics on the wage-floor-level dataset obtained. The final data include 28,907 observations at the wage floor $i \times$ industry agreement $j \times$ year t level, and comprises 316 distinct industry-level agreements j . On average there are 12 wage floors i per industry agreement j and each agreement. The average wage floor annual variation³⁰ is 1.727%. These wage floors display substantial heterogeneity, as displayed in Figure 3.5.3. The index of representativeness has a mean value of 0.885 and a standard deviation of 0.119, which ensures that we have enough variation. We display in Table A1 and Table A2 of the Appendix the ten industries most and least dominated by large firms.

3.5.4 Stylized facts

We present in a first step simple correlations between the employers federations unrepresentativeness and industry's outcomes. Our model predicts that when only large firms' interests are accounted for by employers federations, sector-level agreements are used to impede small firms from existing. Indeed, large firms use this legislative framework to collude in order to secure their position, and to increase their market share. The following stylized facts bring

²⁹The average size of bargaining firms in industry j is computed as follows:

$$L_j = \frac{\sum_{k \in B_j} N_k}{N_{B_j}} \quad (3.20)$$

where B_j is the set of bargaining firms in industry j , N_k the size of firm k , N_{B_j} the number of bargaining firms in industry j . Then to obtain our unrepresentativeness measure, we calculate the percentile p_j such that $F_j(L_j) = p_j$, with F_j the cumulative distribution function of firm size within industry j .

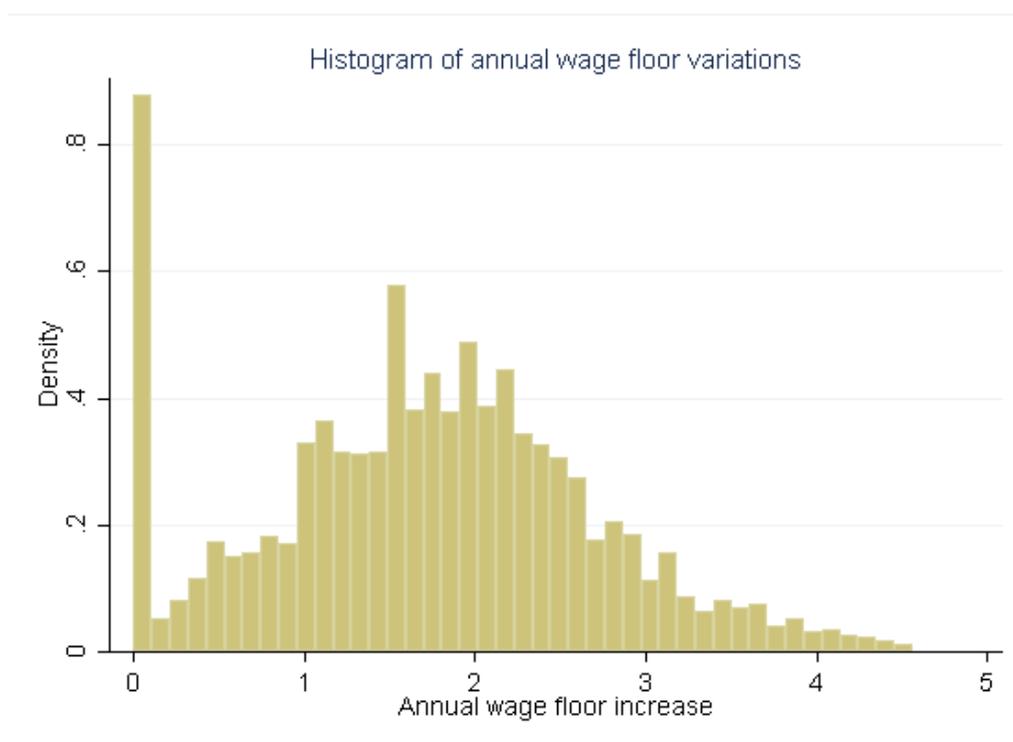
³⁰We compute the wage floor annual variation as follows:

$$\Delta w f_{ijt} = \log\left(\frac{w f_{ijt}}{w f_{ijt-1}}\right) * 100 \quad (3.21)$$

Variable	# Obs.	Mean	Std	Min	Max	Percentiles		
						25 th	50 th	75 th
Wage floor annual evolution(%)	28907	1.727	0.993	0	4.495	1.091	1.737	2.373
Share of employees operating in a small firm	28907	0.378	0.281	0	1	0.153	0.316	0.561
Danish Instrument	28907	0.482	0.178	0	1	0.365	0.465	0.614
German Instrument	28907	0.470	0.209	0	1	0.288	0.443	0.660
Index of representativeness	132	0.885	0.119	0.321	0.996	0.850	0.932	0.982
Number of wage floors per agreement	316	12.337	6.251	2	32	7	11	16

Notes: The wage floor annual evolution represents the evolution, reported as a percentage, of the average value over a year of the wage floor. The share of employees operating in a small firm is computed over the entire population covered by a wage floor and is given by equation 3.19. Danish and German instrument are given by equation 3.24. The index of representativeness is the percentile of firm size distribution of the industry corresponding to size the firms negotiating at the sector-level. The number of wage floors per agreement corresponds to the number of wage floor for each industry-level agreement.

Table 3.5.1 – Summary statistics



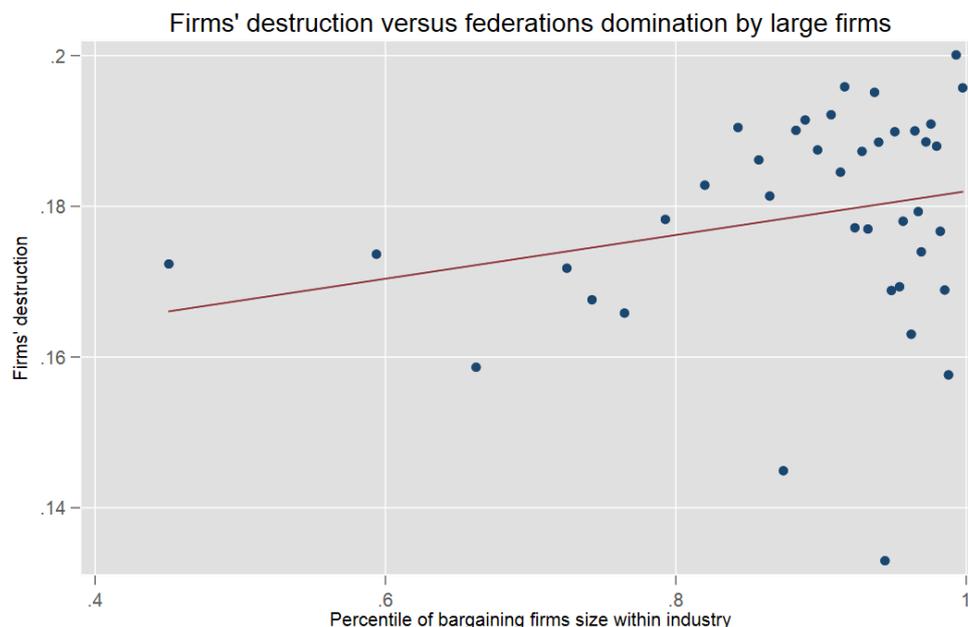
Note: This graph displays the histogram of the evolution of wage floors.

Figure 3.5.3 – Histogram of wage floors variations

evidence of the existence of such a restriction of competition.

Representativeness and small firms destruction Proposition 3.4.1.B first states that the higher the over-representation of large firms interests, the higher the productivity cutoff. We exhibit in Figure 3.5.4 the positive correlation between unrepresentativeness and small firms destruction. As expected, the more interests of large firms are taken into account, the higher the probability that small firms are driven out of the market. This highlights the use

of sector-level agreements by large firms as a barrier to entry.

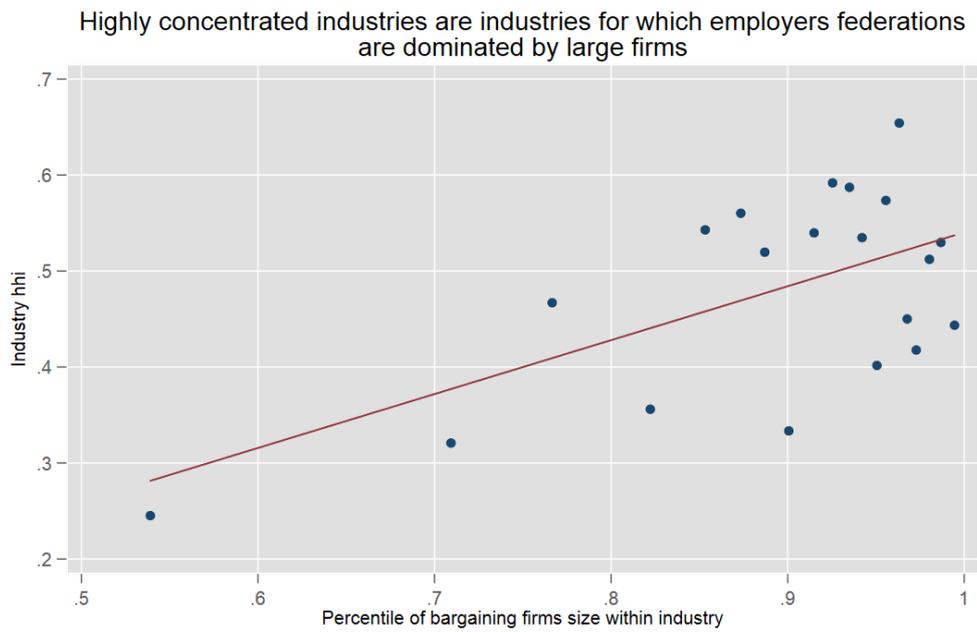


Note: This graph displays the correlation between the proportion of small firms destruction and the unrepresentativeness of the industry. The x-axis represents an index of domination of employers federations by large firms: we compute for each industry the average size of bargaining firms and we calculate the percentile within the industry size distribution that corresponds to this average size. The y-axis is the ratio, computed for each year and each industry, of firms with less than 50 employees that are destroyed over the total number of firms with less than 50 employees.

Figure 3.5.4 – Large firms revenues and federations un-representativeness

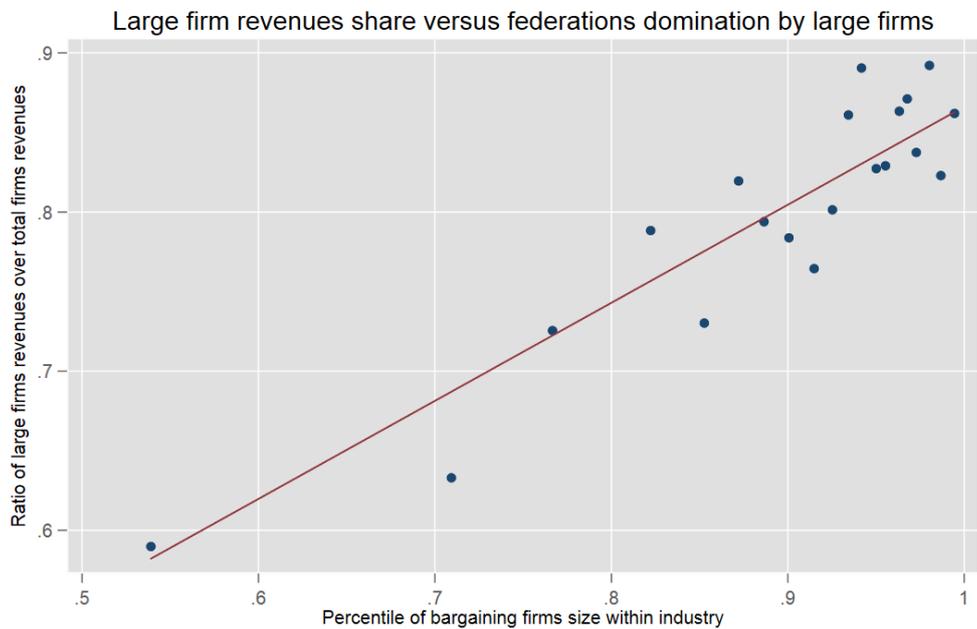
Representativeness and industry concentration Secondly, our theoretical model, in Proposition 3.4.1.B, suggests that the higher the over-representation of large firms in employers federations, the lower the product market concentration. We proxy product market competition by the industry Herfindahl index, denoted hhi in the following.³¹ The higher the hhi , the higher the concentration of the industry. Figure 3.5.5 exhibits the positive correlation between the hhi and the over-representation of large firms interests in federations. Moreover, the same Proposition implies that large firms' revenues increase with the unrepresentativeness of employers federations. We display in Figure 3.5.6 the correlation between unrepresentativeness and the share of the industry's revenues captured by large firms.

³¹The Herfindahl index for an industry j is constructed as the sum of the squared market shares of firms i of the industry. It is algebraically as follows: $\sum_i \left(\frac{sales_i}{\sum_i sales_i} \right)^2$



Note: This graph displays the correlation between industry concentration, as measured by the Herfindahl index (hhi) of sales in each industry, and the unrepresentativeness of the industry. The x-axis represents an index of domination of employers federations by large firms: we compute for each industry the average size of bargaining firms and we calculate the percentile within the industry size distribution that corresponds to this average size.

Figure 3.5.5 – Industry concentration and federations un-representativeness



Note: This graph displays the correlation between the share of large firms revenues in total industry revenues and the unrepresentativeness of the industry. The x-axis represents an index of domination of employers federations by large firms: we compute for each industry the average size of bargaining firms and we calculate the percentile within the industry size distribution that corresponds to this average size. The y-axis is the ratio of large firms revenues (large firms being firms over the 75th percentile in employment level) in total industry revenues.

Figure 3.5.6 – Large firms revenues and federations un-representativeness

3.5.5 Estimation strategy

The stylized facts displayed above are indicative about the correlation between federations' representativeness and industry concentration. The theoretical mechanism explaining this correlation is that bargaining firms have differential incentives to raise wage floors whether they are representative or not of the average firm in the industry. We test this theoretical prediction by studying the effect of the share of small firms on bargained wages, both for representative and unrepresentative industries.

OLS estimation In order to test the validity of theoretical conclusions, we test whether in unrepresentative industries the share of workers operating in small firms is positively correlated to wage floor variations. Formally, we estimate the following equation:

$$\Delta w f_{ijt} = \beta_0 + \gamma_1 S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j) + \gamma_2 S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j) + \theta_o + \zeta_{jt} + \epsilon_{ijt} \quad (3.22)$$

Where $\Delta w f_{ijt}$ is the evolution of the wage floor i , negotiated as part of the industry level agreement j , between year t and $t - 1$. S_{ijt} denotes the share of workers in wage floor level i and industry agreement j operating in small firms.³² U_j is our industry-level indicator of federations unrepresentativeness and \bar{U}_j the median value of unrepresentativeness in our sample. Industries with a high value of U_j are therefore industries whose employers federations over-represent large firms the most - *i.e.* unrepresentative industries. γ_1 therefore captures the effect of the share of workers employed in small firms for unrepresentative industries, and γ_2 for representative industries. θ_o and ζ_{jt} respectively denote occupation fixed effects and industry-level \times year fixed effects, and ϵ_{ijt} the error term.

One should note that looking at wage floor variations, rather than wage floor levels, is crucial for the analysis as it mitigates the potential reverse causality issue. The theoretical model indeed states that high wage floors lead to an eviction of small firms, thus inducing a negative correlation between wage floor levels and the number of small firms. However, this effect plausibly unfolds in the medium or long term. In the short term, as the share of small firms employees increases, large firms' incentives to raise wage floors increase as well: this should induce a positive correlation between wage floor variations and the share of small firms.

³²For a formal definition of S_{ijt} , see equation 3.19

To consistently estimate $\hat{\gamma}_1$ and $\hat{\gamma}_2$, one requires exogeneity, *i.e.*

$$E(\epsilon_{ijt} | S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j), S_{ijt} \times \mathbb{1}(U_j \leq \bar{U}_j)) = 0 \quad (3.23)$$

However, it is highly likely that $E(\epsilon_{ijt} | S_{ijt}) \neq 0$. Indeed, endogeneity may stem from the following sources. First, industry-level productivity shocks could be correlated with both the share of workers employed by small firms and the negotiated wage floors. To alleviate this concern, we add industry agreement \times year fixed effects to remove both time-constant and time-varying unobserved industry-level heterogeneity. We therefore exploit the within industry \times year variation, namely the variation across wage floor skill-levels i . Yet, the wage floor skill-level i - *i.e.* the ranking of the given wage floor amongst all wage floors of an industry agreement and year jt - is naturally correlated to the socio-professional category of the worker. Indeed, the higher the wage floor skill-level i , the higher the hierarchy of the worker in the firm. Within industry \times year, the unobserved productivity of an occupation could impact both the share of workers in small firms and the negotiated wage floors i . To mitigate this issue, we add socio-professional category fixed effects θ_o that broadly capture the unobserved variables affecting the demand for some type of workers.

2SLS estimation Despite the inclusion of various fixed effects, the OLS estimation may still suffer from endogeneity bias. For instance, the model predicts that the wage floor level has a negative impact on small firms survival, and therefore on the share of workers employed by small firms. The inclusion of fixed effects does not fix this reverse causality issue. The resulting negative correlation between past wage floor variations, uncontrolled for, and the current share of workers employed in small firms would in turn create an omitted variable bias.

In order to circumvent this endogeneity issue, we construct "shift-share" variables (see Bartik (1991), Borusyak and Jaravel (2017a) and Goldsmith-Pinkham, Sorkin and Swift (2018)) thanks to the share of workers employed by small firms in Denmark and Germany.³³ Because both $S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$ and $S_{ijt} \times \mathbb{1}(U_j < \bar{R}_j)$ are endogenous in equation 3.22, we need to construct at least two instrumental variables. To construct the instruments, we exploit the co-existence within industry agreement j of several 3-digit level sectors s . Workers covered by a given wage floor i within industry agreement j may pertain to different sectors s .³⁴ We

³³We chose Denmark and Germany for our instrumental strategy mainly because of data availability and precision.

³⁴For instance, industry agreement number 18 - textile industry - covers among others workers from the 'weaving' sector (sector number 1320Z), the 'textile retailing for specialized shops' sector (sector number 4751Z)

compute for each 3-digit level sector s , country $c \in (D, G)$ and year t , the share T_{cst} of workers working in small firms. We then compute, for each country $c \in (D, G)$, the instrument I_{cijt} for each wage floor i within industry agreement j and year t as the weighted average of these sectoral shares. For each country $c \in (D, G)$, the instrument writes:

$$I_{cijt} = \sum_{s=1}^S \omega_{ijst} T_{cst} \quad (3.24)$$

where ω_{ijst} is the share of French workers of wage floor i within industry agreement j in year t pertaining to the 3-digit sector s , T_{cst} the share in country c , sector s and year t of workers working in small firms and S the total number of sectors.

Our aim is to instrument $S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$ and $S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$ by the Danish and German instruments I_{Dijt} and I_{Gijt} . These Bartik-like instruments thus represent the propensity of a particular skill level i within agreement j to be demanded by small firms. For the instrument to be valid, two conditions must hold: the relevance condition and the exclusion restriction assumption.

The relevance condition states that each instrument I_{cijt} should be correlated with the instrumented variables $S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$ and $S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$. The intuition behind the construction of our instrument is that sectoral firm size distributions are correlated across countries through a common production technology, thereby inducing a correlation between I_{cijt} and S_{ijt} . This relevance assumption can be directly tested: we display in columns (3) and (5) of Table 3.5.2 the Kleibergen-Paap test statistics for weak instruments. Because tests for weak instruments in a multiple endogenous regressors setting have not yet been formally derived (see Andrews, Stock and Sun (2018)), we report in the robustness section an Anderson-Rubin test (see Anderson, Rubin et al. (1949)), robust to weak instruments.

The exclusion restriction writes: $E(\epsilon_{ijt} | I_{Dijt}, I_{Gijt}) = 0$. It states that the errors from the equation of interest - equation 3.22 - should be independent from the share of workers employed by small firms in Denmark and Germany. In economic terms: we want the Danish sectoral firm size distribution to be uncorrelated with any unobserved factor affecting the French wage bargaining outcomes.

Two conditions are necessary for the exclusion restriction to hold. First, Danish and German sectoral shares T_{st} should not be correlated with a given wage floor-industry agreement pair (i, j) unobserved characteristics. A pair (i, j) 's unobserved characteristics could include

and the 'pre-press' sector (sector number 1813Z).

It must be noted though that 3-digit sectors s are not nested within industry agreement j , which differentiates our instrument from standard Bartik instruments.

among others comparative advantage, productivity or past bargaining outcomes. Naturally Danish and German sectoral shares T_{st} are correlated with industry j unobserved comparative advantage. However, industry \times year fixed effects capture such an unobserved comparative advantage, thereby alleviating the endogeneity issue. Another potential threat to identification could be the existence of a technological shock hitting small firms that would be common to France and the country of interest (either Denmark or Germany). Such technological shock would induce a change in the French labor demand by small firms, thus probably changing the wage floors, and would be correlated to both the Danish (for instance) share of workers employed in small firms. We alleviate this concern by controlling for the evolution of the share of workers employed in small firms. Controlling for such evolutions indeed enables to capture labor demand shocks that would affect proportionally more small firms. The conjunction of foreign - Danish and German - data, industry \times year fixed effects and controls of evolution of the share of workers employed in small firms therefore mitigates the concern that sectoral averages T_{st} could be correlated to a given wage floor-industry agreement pair (i, j) unobserved characteristics.

Second, the weights w_{ijst} capture the sectoral heterogeneity of each wage floor-industry agreement pair (i, j) . In order for the exclusion restriction to hold, such heterogeneity should not capture other factors that might affect the wage bargaining outcomes. Reverse causality in particular could lead to biased estimates. A wage bargaining agreement in year $t - 1$ for wage floor i and industry j that increases the wage floor level might have different employment consequences on sectors s and s' in year t . For example, if sector s is initially less productive than sector s' , sector s suffers from a greater employment loss in t as compared to sector s' . Such differential employment consequences would lead to a decrease of the weight w_{ijst} in year t . Hence, the sectoral heterogeneity of each wage floor-industry agreement pair (i, j) may capture past wage bargaining outcomes, and may thus affect the wage bargaining outcome in year t . To alleviate this concern, we use lagged values of the instruments $I_{D,ijt}$ and $I_{G,ijt}$.

One caveat is that I_{Dijt} and I_{Gijt} are highly correlated - because they are constructed with the same weights w_{ijst} -, which may lead to a weakly identified model. To reduce the correlation between our two instruments, we construct the German instrument thanks to 3-digit sectors s , while we build the Danish instrument with 2-digit sectors s .³⁵ This guarantees that our two instruments add sufficiently different variation in order to identify the effects of interest.

³⁵Data availability precludes us from testing the opposite, *i.e.* from constructing the German instrument thanks to 2-digit sectors and the Danish instrument with 3-digit sectors

3.5.6 Results

Table 3.5.2 displays IV estimations of equation 3.22, *i.e.* the effect of the share of workers working in small firms on wage floor variations for representative and unrepresentative industries. Column (1) displays the results when controlling only for industry \times year fixed effects, column (2) adds occupation fixed-effects, column (3) controls for the lagged value of the wage floor evolution and column (4) for the second lag of the wage floor evolution. Regardless of the controls and fixed effects used, the share of workers operating in small firms has a significant positive effect on wage floor variations only for unrepresentative industries. Column (4) indicates that, for those industries, an increase of the share from 0 to 1 increases the wage floor variations by 1.07 percentage points. For representative industries, no significant effect is found thereby confirming the role of unrepresentativeness for wage floor variations.

Table 3.5.2 – The role of unrepresentativeness on wage floor increase (in %)

	Wage floor increase (%)				
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	t-1	t-1	t-1	t-1	t-1
$S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$	1.094** (0.409)	1.089** (0.409)	1.068** (0.409)	1.071** (0.410)	0.855* (0.442)
$S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$	-0.132 (0.509)	-0.0144 (0.513)	0.162 (0.540)	0.122 (0.514)	0.167 (0.767)
ΔS_{ijt}				-0.00771 (0.0116)	-0.00644 (0.0148)
ΔS_{ijt-1}					0.00558 (0.00764)
Cragg-Donald F-stat	76.08	82.39	75.70	89.56	41.57
Kleibergen-Paap F-stat	4.88	5.58	5.98	7.13	4.08
# obs	9445	9440	9438	9370	9252
Occupation FE	No	Yes	No	No	No
Occupation \times Year FE	No	No	Yes	Yes	Yes
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the wage floor variation between $t - 1$ and t . S_{ijt} denotes the share of workers employed in small firms for the skill-level position i , industry j and year t . $\mathbb{1}(U_j > \bar{U}_j)$ is a dummy equal to one when the industry j is less representative than the median industry: U_j is a measure of unrepresentativeness (the larger U_j , the higher the domination of the employers federations by large firms), and \bar{U}_j is the median unrepresentativeness. Columns (1) exhibits the IV estimation of equation 3.22 when only industry \times year fixed effects are used, columns (2) and (3) display the results when adding respectively occupation and occupation \times year fixed effects, column (4) exhibits the results when controlling for the evolution of the share of workers employed in small firms and column (5) when controlling for both the evolution and lagged evolution of the share of workers employed in small firms. Two instruments are used in all regressions: the lagged value of the Danish and German instruments. The Danish and German instruments are constructed as Bartik-like instrument thanks to the share of workers employed in small firms in each sector in Denmark and Germany. Standard errors are clustered at the industry \times year level. Standard errors are given in brackets. *, **, and *** denote statistical significance at 10, 5 and 1%.

3.5.7 Robustness

We conduct a full range of robustness checks to confirm the validity of our findings.

First, we address the issue of potentially-weak instruments. To the best of our knowledge, tests for weak identification in case of multiple endogenous regressors and heteroskedastic errors have yet not been formally derived (Olea and Pflueger (2013), Andrews et al. (2018)). However, because our model is just-identified, we can provide confidence sets that remain valid whether or not the instruments are weak. Namely, we perform an Anderson-Rubin test to test whether our two potentially-weakly identified endogenous regressors are jointly significant (Andrews et al. (2018)). The p-value of the Anderson-Rubin test for joint insignificance being equal to 0.030³⁶, we reject the null hypothesis of joint insignificance, thus confirming that the effect found in our main specifications is not driven by weak instruments bias.

Second, we perform the IV estimation of equation 3.22 by using the second lag of both instruments. We report such results in Table 3.5.3. As in the main specification, the share of workers employed in small firms has a significant and positive impact only for unrepresentative industries. We also report results with no lag in Table 3.5.4.

Third, we use several different definitions of unrepresentative industries: columns (1) and (2) of Table 3.5.5 display the results when defining the unrepresentative industries not as the industries less representative than the median industry, but less representative than two thirds of the industries - *i.e.* with another unrepresentativeness threshold \bar{U}_j . Columns (3) and (4) of Table 3.5.5 display the results using the ratio of bargaining firm size over total firm size within the industry as the unrepresentativeness measure. The higher this measure, the larger the bargaining firms as compared to the other firms of the industry, therefore the larger the domination of the employers federations by large firms. Defining representative industries with these criteria does not substantially change the results: in all the estimations, we find a significant and positive effect of the share of workers employed in small firms only in unrepresentative industries. The magnitudes of the estimates vary only little.

Fourth, we use an alternative definition of small firms. Throughout the analysis we consider a small firm as a firm with less than 50 employees. We perform the estimations when considering a firm small when having less than 100 employees. We display in Table 3.5.6 the estimates when using this new definition. As for our previous definition, we display the results with a various range of controls and fixed effects. The results are not sensitive at all to the change of small firm definition: the effect of the share of workers employed in small firms is once again only significant for unrepresentative industries, and the magnitudes are very similar

³⁶The p-value of the Wald test, *i.e.* not robust to weak instruments, is slightly lower and equal to 0.022.

to the one found with the main specification - *i.e.* very similar to Table 3.5.2.

Table 3.5.3 – The role of representativeness on wage floor increase (in %) - robustness 1 - using second lag of instrument

	Wage floor increase (%)				
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	t-2	t-2	t-2	t-2	t-2
$S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$	0.929*	0.804	0.849*	0.901*	0.908*
	(0.532)	(0.509)	(0.496)	(0.505)	(0.506)
$S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$	-0.381	-0.209	-0.136	-0.105	-0.0869
	(1.002)	(0.916)	(0.876)	(0.861)	(0.852)
ΔS_{ijt}				-0.00237	-0.00174
				(0.0165)	(0.0178)
ΔS_{ijt-1}					0.00695
					(0.00775)
Kleibergen-Paap	1.53	1.98	2.22	3.15	3.04
# obs	9451	9447	9444	9323	9278
Occupation FE	No	Yes	No	No	No
Occupation \times Year FE	No	No	Yes	Yes	Yes
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the wage floor variation between $t - 1$ and t . S_{ijt} denotes the share of workers employed in small firms for the skill-level position i , industry j and year t . $\mathbb{1}(U_j > \bar{U}_j)$ is a dummy equal to one when the industry j is less representative than the median industry: U_j is a measure of unrepresentativeness (the larger U_j , the higher the domination of the employers federations by large firms), and \bar{U}_j is the median unrepresentativeness. Columns (1) exhibits the IV estimation of equation 3.22 when only industry \times year fixed effects are used, columns (2) and (3) display the results when adding respectively occupation and occupation \times year fixed effects, column (4) exhibits the results when controlling for the evolution of the share of workers employed in small firms and column (5) when controlling for both the evolution and lagged evolution of the share of workers employed in small firms. Two instruments are used in all regressions: the second lagged value of the Danish and German instruments. The Danish and German instruments are constructed as Bartik-like instrument thanks to the share of workers employed in small firms in each sector in Denmark and Germany. Standard errors are clustered at the industry \times year level. Standard errors are given in brackets. *, **, and *** denote statistical significance at 10, 5 and 1%.

Table 3.5.4 – The role of representativeness on wage floor increase (in %) - robustness 1 - using no lag for instrument

	Wage floor increase (%)				
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
	t	t	t	t	t
$S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$	0.873** (0.385)	0.738** (0.368)	0.735** (0.358)	0.798** (0.371)	0.809** (0.350)
$S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$	-0.206 (0.283)	-0.137 (0.266)	-0.159 (0.268)	-0.179 (0.323)	-0.148 (0.414)
ΔS_{ijt}				-0.000618 (0.00896)	-0.000932 (0.0106)
ΔS_{ijt-1}					0.00659 (0.00617)
Kleibergen-Paap	11.55	14.36	15.47	12.15	8.20
# obs	9451	9491	9487	9485	9238
Occupation FE	No	Yes	No	No	No
Occupation \times Year FE	No	No	Yes	Yes	Yes
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the wage floor variation between $t - 1$ and t . S_{ijt} denotes the share of workers employed in small firms for the skill-level position i , industry j and year t . $\mathbb{1}(U_j > \bar{U}_j)$ is a dummy equal to one when the industry j is less representative than the median industry: U_j is a measure of unrepresentativeness (the larger U_j , the higher the domination of the employers federations by large firms), and \bar{U}_j is the median unrepresentativeness. Columns (1) exhibits the IV estimation of equation 3.22 when only industry \times year fixed effects are used, columns (2) and (3) display the results when adding respectively occupation and occupation \times year fixed effects, column (4) exhibits the results when controlling for the evolution of the share of workers employed in small firms and column (5) when controlling for both the evolution and lagged evolution of the share of workers employed in small firms. Two instruments are used in all regressions: the current values of the Danish and German instruments. The Danish and German instruments are constructed as Bartik-like instrument thanks to the share of workers employed in small firms in each sector in Denmark and Germany. Standard errors are clustered at the industry \times year level. Standard errors are given in brackets. *, **, and *** denote statistical significance at 10, 5 and 1%.

Table 3.5.5 – The role of representativeness on wage floor increase (in %) - robustness 2 - using other definitions of representativeness \widetilde{U}_j

	Wage floor increase (%)			
	(1) IV	(2) IV	(3) IV	(4) IV
	measure 1 of repres t-1	measure 1 of repres t-2	measure 2 of repres t-1	measure 2 of repres t-2
$S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$	1.245** (0.523)	1.103 (0.675)	1.115** (0.438)	0.919* (0.519)
$S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$	0.123 (0.524)	-0.196 (0.982)	0.0835 (0.543)	-0.129 (0.893)
ΔS_{ijt}	-0.00693 (0.0121)	0.000264 (0.0191)	-0.00917 (0.0112)	-0.00393 (0.0155)
Kleibergen-Paap # obs	6.51 9370	2.13 9323	6.13 9370	2.87 9323
Occupation \times Year FE	Yes	Yes	Yes	Yes
Industry \times Year FE	Yes	Yes	Yes	Yes

Note: The dependent variable is the wage floor variation between $t - 1$ and t . S_{ijt} denotes the share of workers employed in small firms for the skill-level position i , industry j and year t . $\mathbb{1}(U_j > \bar{U}_j)$ is a dummy equal to one when the industry j is less representative than the median industry: U_j is a measure of unrepresentativeness (the larger U_j , the higher the domination of the employers federations by large firms), and \bar{U}_j is the median unrepresentativeness. Columns (1) and (2) exhibit the IV estimation of equation 3.22 when considering unrepresentative industries as industries in the top tercile of the unrepresentativeness measure. Columns (3) and (4) exhibit the IV estimation of equation 3.22 using as an unrepresentativeness measure the ratio of the average bargaining firm size and the average firm size in the industry. The instruments used are Danish and German instruments are constructed as Bartik-like instrument thanks to the share of workers employed in small firms in each sector in Denmark and Germany. Industry \times Year fixed effects and occupation fixed effects are included in all regressions. Standard errors are clustered at the industry \times year level. Standard errors are given in brackets. *, **, and *** denote statistical significance at 10, 5 and 1%.

Table 3.5.6 – The role of representativeness on wage floor increase (in %) - robustness 3 - using different definition of small firm

	Wage floor increase (%)				
	(1)	(2)	(3)	(4)	(5)
	IV t-1	IV t-1	IV t-1	IV t-1	IV t-1
$S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$	0.994** (0.392)	0.975** (0.374)	0.936** (0.371)	0.935** (0.373)	0.742** (0.368)
$S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$	0.0575 (0.740)	0.249 (0.756)	0.517 (0.805)	0.439 (0.756)	0.526 (0.916)
ΔS_{ijt}				-0.0219 (0.0185)	-0.0227 (0.0204)
ΔS_{ijt-1}					-0.00640 (0.00585)
Kleibergen-Paap	4.16	5.32	5.05	5.67	3.79
# obs	9445	9440	9438	9415	9322
Occupation FE	No	Yes	No	No	No
Occupation \times Year FE	No	No	Yes	Yes	Yes
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the wage floor variation between $t - 1$ and t . S_{ijt} denotes the share of workers employed in small firms for the skill-level position i , industry j and year t . $\mathbb{1}(U_j > \bar{U}_j)$ is a dummy equal to one when the industry j is less representative than the median industry: U_j is a measure of unrepresentativeness (the larger U_j , the higher the domination of the employers federations by large firms), and \bar{U}_j is the median unrepresentativeness. Columns (1), (2) and (3) exhibit respectively the OLS estimation of equation 3.22, the IV estimation of equation 3.22 using the Danish instrument, and the IV estimation of equation 3.22 using the German instrument. The Danish and German instruments are constructed as Bartik-like instrument thanks to the share of workers employed in small firms in each sector in Denmark and Germany. Industry \times Year fixed effects and occupation fixed effects are included in all regressions. Standard errors are clustered at the industry \times year level. Standard errors are given in brackets. *, **, and *** denote statistical significance at 10, 5 and 1%.

3.6 Conclusion

This chapter sheds light on the effect of industry-level wage bargaining on product market competition. We first show theoretically that large firms use collective bargaining as a tool to drive small firms out of the market, thereby decreasing employment and consumers' utility. Furthermore, the higher the over-representation of large firms in employers federations, the higher these cartel effects. Indeed, the larger the domination of large firms, the larger the bargained wage floors, which entails in turn an increased eviction of small firms from the product and labor markets.

In order to confirm those theoretical predictions, we use French administrative data and wage floors information. First, we study the cartel effect by looking at the impact of the share of small firms employees among covered workers on the bargained wage floors increases. The higher this share, the higher the incentives for large firms to raise wage floors. Indeed, when this share is high, the negative impact of wage floors is borne proportionally more by small firms. On this basis, we compute, for each wage floor, the ratio of covered workers operating in small firms to the total number of covered workers. We find that it has a positive and significant effect on the percentage of annual increase of the wage floors. Second, our novel indices of the large firms' domination within federations prove to be positively correlated with the variation of negotiated wages, thereby corroborating the model's predictions. We next devise an instrumental strategy that will enable us to move from correlational to causal evidence.

Consequently, the representativeness of employers federations may be an interesting lever for policy-makers who wish to increase competitiveness in particular sectors. We also encourage policy-makers to further develop their data collection regarding federations representativeness.

3.7 Appendices

3.7.1 Appendix A: Collective bargaining in OECD countries

Countries ordered by predominant level of collective bargaining, degree of centralisation, co-ordination, trade union density in the private sector, collective bargaining coverage, employer organisation density and quality of labour relations

	Predominant level	Degree of centralisation/ decentralisation	Co-ordination	Trade union density in the private sector	Employer's organisation density	Collective bargaining coverage rate	Quality of labour relations
Costa Rica	Company	Decentralised	No	Less than 5%	--	5-10%	--
Colombia	Company	Decentralised	No	Less than 5%	--	5-10%	Low
Turkey	Company	Decentralised	No	Less than 5%	20-30%	5-10%	Low
Estonia	Company	Decentralised	No	Less than 5%	20-30%	10-20%	High
Lithuania	Company	Decentralised	No	5-10%	10-20%	5-10%	Medium
Mexico	Company	Decentralised	No	5-10%	--	10-20%	Low
United States	Company	Decentralised	No	5-10%	--	10-20%	Medium
Korea	Company	Decentralised	No	5-10%	10-20%	10-20%	Low
Poland	Company	Decentralised	No	5-10%	20-30%	10-20%	Low
Latvia	Company	Decentralised	No	5-10%	40-50%	10-20%	Medium
Hungary	Company	Decentralised	No	5-10%	40-50%	20-30%	Medium
Chile	Company	Decentralised	No	10-20%	--	10-20%	Medium
New Zealand	Company	Decentralised	No	10-20%	--	10-20%	Medium
Canada	Company	Decentralised	No	10-20%	--	20-30%	Medium
United Kingdom	Company	Decentralised	No	10-20%	30-40%	20-30%	Medium
Czech Republic	Company	Decentralised	No	10-20%	60-70%	40-50%	High
Ireland	Company	Decentralised	No	20-30%	50-60%	40-50%	Medium
Japan	Company	Decentralised	High	10-20%	--	10-20%	High
Israel	Company/Sectoral	Decentralised	No	10-20%	--	20-30%	Low
Slovak Republic	Company/Sectoral	Decentralised	No	10-20%	30-40%	20-30%	Medium
Greece	Company/Sectoral	Decentralised	No	10-20%	40-50%	40-50%	Low
Australia ³	Company/Sectoral	Decentralised	No	10-20%	--	50-60%	Low
Luxembourg	Company/Sectoral	Decentralised	No	20-30%	80-90%	50-60%	High
Spain	Sectoral	Organised decentralised	Low	10-20%	70-80%	70-80%	Low
Switzerland	Sectoral	Organised decentralised	High	10-20%	--	40-50%	High
Germany	Sectoral	Organised decentralised	High	10-20%	50-60%	50-60%	High
Netherlands	Sectoral	Organised decentralised	High	10-20%	80-90%	80-90%	High
Austria	Sectoral	Organised decentralised	High	20-30%	90% or more	90% or more	High
Norway	Sectoral	Organised decentralised	High	30-40%	60-70%	60-70%	High
Denmark	Sectoral	Organised decentralised	High	60-70%	60-70%	80-90%	High
Sweden	Sectoral	Organised decentralised	High	60-70%	80-90%	90% or more	High
Slovenia	Sectoral	Centralised	No	10-20%	60-70%	60-70%	Low
Iceland	Sectoral	Centralised	No	80-90%	60-70%	80-90%	High
France	Sectoral	Centralised	Low	5-10%	70-80%	90% or more	Medium
Portugal	Sectoral	Centralised	Low	10-20%	30-40%	60-70%	Medium
Italy	Sectoral	Centralised	Low	20-30%	50-60%	80-90%	Low
Finland	Sectoral/National	Centralised	High	50-60%	70-80%	80-90%	High
Belgium	Sectoral/National	Centralised	High	50-60%	80-90%	90% or more	Medium

Figure 3.7.1 – Dashboard of collective bargaining systems, 2015

3.7.2 Appendix B: Aggregate variables

The formulas for the aggregate price index, P_j , the aggregate profit, Π_j , the aggregate revenue, R_j , and the aggregate employment, L_j , are given by :

$$P_j = \left[\frac{1}{1 - G(\phi_j^*)} \int_{\phi_j^*}^{\infty} p^{1-\sigma}(\phi) M_j g(\phi) d\phi \right]^{\frac{1}{1-\sigma}} \quad (3.25)$$

$$R_j = \frac{1}{1 - G(\phi_j^*)} \int_{\phi_j^*}^{\infty} r(\phi) M_j g(\phi) d\phi = M_j \bar{r}_j \quad (3.26)$$

$$\Pi_j = \frac{1}{1 - G(\phi_j^*)} \int_{\phi_j^*}^{\infty} [\pi(\phi) M_j g(\phi) d\phi] = M_j \left[\frac{\bar{r}_j}{\sigma} - f \right] \quad (3.27)$$

$$L_j = \frac{1}{1 - G(\phi_j^*)} \int_{\phi_j^*}^{\infty} \frac{q_j(\phi)}{\phi} M_j g(\phi) d\phi \quad (3.28)$$

3.7.3 Appendix C: Firm-level bargaining

Proof of Proposition 3.3.1

Proposition 3.3.1.B Revenues are increasing with productivity Equation 3.5 implies

$$\frac{\partial q(\phi, w)}{\partial w} = -\sigma \frac{\partial p(\phi, w)}{\partial w} \frac{q(\phi, w)}{p(\phi, w)} = -\sigma \frac{q(\phi, w)}{w}$$

Using equation 3.8, we can derive

$$\frac{1}{\pi(\phi, w)} \frac{\partial \pi(\phi, w)}{\partial w} = \frac{(1 - \sigma) r(\phi, w)}{w(r(\phi, w) - \sigma f)}$$

Which directly implies that the wage is given by the following equation

$$\frac{-\beta\sigma}{w} + \frac{\beta}{w - \tilde{w}} + \frac{(1 - \beta)(1 - \sigma) r(\phi, w)}{w(r(\phi, w) - \sigma f)} = 0 \quad (3.29)$$

Let's suppose that there exists ϕ_1 and ϕ_2 such that $\phi_1 < \phi_2$ and $\pi(\phi_1) > \pi(\phi_2)$. The previous equation implies that the wage is equal to

$$w = \tilde{w} \left[1 + \frac{\beta}{(\sigma - 1)(1 - \beta) \frac{r(\phi, w)}{r(\phi, w) - \sigma f} + \beta(\sigma - 1)} \right] \quad (3.30)$$

This equation implies that, if $\pi(\phi_1) > \pi(\phi_2)$, then $w(\phi_1) > w(\phi_2)$. Therefore, using equations 3.6 and 3.7, $r(\phi_1) > r(\phi_2)$ holds if and only if $\phi_1 > \phi_2$, which is a contradiction. This directly implies that the revenue is non-decreasing functions of productivity.

Proposition 3.3.1.A Wage is increasing with productivity Because the revenue is non-decreasing in productivity, equation 3.30 proves that wage is also non-decreasing in productivity.

Moreover, a firm produces if and only if its profits are positive. This gives the lower bound of the wage, equal to \tilde{w} . Finally, values of productivity are in the range of 0 to infinity, which directly implies that profits of firms tend toward infinity.

Using the wage equation 3.30 we derive the higher bound of the wage

$$w_f^{max} = \lim_{\phi \rightarrow \infty} w_f(\phi) = \tilde{w} \left[1 + \frac{\beta}{\sigma - 1} \right] \quad (3.31)$$

3.7.4 Appendix D: Industry-level bargaining

Equilibrium structure of the industry

We first introduce Proposition 3.7.1 and its proof in order to be able then to prove Proposition 3.3.2.

Proposition 3.7.1

Using definition of equation 3.9, it can be derived that :

Proposition 3.7.1 *When an industry wage floor is implemented, the average profit is equal to $\bar{\pi}_i = f [\Gamma_i(\phi^*, \bar{w}) - 1]$ where $\Gamma_i(\phi^*, \bar{w})$ fulfills the following conditions*

$$\begin{cases} \Gamma_i(\phi^*, \bar{w}) = \Gamma_f(\phi^*) \text{ if } \bar{w} \leq \tilde{w} \\ \Gamma_i(\phi^*, \bar{w}) > \Gamma_f(\phi^*) \text{ and } \frac{\partial \Gamma_i(\phi^*, \bar{w})}{\partial \bar{w}} > 0 \text{ if } \tilde{w} < \bar{w} < w_f^{max} \\ \Gamma_i(\phi^*, \bar{w}) = \Gamma_i^{max}(\phi^*) = \frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \left[\frac{\phi^*}{\phi} \right]^{1-\sigma} g(\phi) d\phi \text{ if } \bar{w} \geq w_f^{max} \end{cases} \quad (3.32)$$

First, if the industry's minimum wage is inferior to the reservation wage, it will be lower than the lowest wage negotiated at the firm-level, implying that it has no impact on the zero cutoff profit condition.

If the industry wage floor is superior to the reservation wage it will increase the labor cost of the less productive firms. For a given ϕ^* , this wage floor will increase the labor cost of the firm at the productivity cutoff, and will not be binding for the largest companies. As a consequence, the decrease of the labor cost per product, with respect to productivity, will be more significant. Consequently, a rise in the labor cost of less productive firms will increase profits of the most productive ones and so increase the value of the average profit. This implies that, for a given value of the productivity cutoff, there is a positive relation between the average profit and the wage floor.

Finally there is an upper bound reached when the industry-minimum wage is binding for every operating firm.

Proof of Proposition 3.7.1

We study the following function.

$$\Gamma_i(\phi^*, \bar{w}) = \frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma} g(\phi) d\phi$$

The situation where the wage floor is lower than the reservation wage is trivial, therefore we focus on the situation where $\tilde{w} < \bar{w} < w_f^{max}$, and we compare, for any value of ϕ^* , $\Gamma_i(\phi^*, \bar{w})$ and $\Gamma_f(\phi^*)$.

First, we prove that the revenue of firms paying a wage equal to the wage floor increases. We define $\tilde{\phi}_i(\phi^*, \bar{w})$ as the higher productivity level paying a wage equal to the wage floor. For any firm with productivity respecting the condition $\phi \in]\phi^*; \tilde{\phi}(\phi^*, \bar{w})]$, its revenue is equal to $\left[\frac{\phi^*}{\phi}\right]^{1-\sigma} \sigma f > \left[\frac{w_f(\phi)\phi^*}{\bar{w}\phi}\right]^{1-\sigma} \sigma f$.

Secondly, we prove that firms paying a wage higher than the wage floor have a higher revenue. Indeed, let's suppose that the revenue of firms with a productivity level higher than $\tilde{\phi}(\phi^*, \bar{w})$ decreases. Using equation 3.29, this would directly imply that $w_i(\phi, \bar{w}) < w_f(\phi)$. However, as the profit of firms at the productivity cutoff doesn't change and as the wage paid by those firms increases, equation 3.8 would directly imply that the revenue of firms with a productivity level higher than $\tilde{\phi}(\phi^*, \bar{w})$ would increase. This contradiction proves that the revenue of those firms must increase when there is a wage floor.

Building on previous insights, it can easily be derived that, for any value of ϕ^* , if the wage floor is strictly higher than \tilde{w}

$$\Gamma_i(\phi^*, \bar{w}) = \frac{1}{1 - G(\phi^*)} \left[\int_{\phi^*}^{\tilde{\phi}_i(\phi^*, \bar{w})} \left(\frac{\phi^*}{\phi}\right)^{1-\sigma} g(\phi) d\phi + \int_{\tilde{\phi}_i(\phi^*, \bar{w})}^{\infty} \frac{r_i(\phi, \bar{w})}{\sigma f} g(\phi) d\phi \right] > \Gamma_f(\phi^*)$$

Finally, it is straightforward to derive that, when the wage is greater or equal to w_f^{max} , then

$$\Gamma_i^{max}(\phi^*) = \frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \left[\frac{\phi^*}{\phi}\right]^{1-\sigma} g(\phi) d\phi$$

Proof of Proposition 3.3.2

Proposition 3.3.2.A states that : *As long as the wage floor is between \tilde{w} and w_f^{max} there is a strictly positive relation between the average revenue and the wage floor, and the same applies to the productivity cutoff.*

Furthermore, the average revenue fulfills the following conditions.

$$\left\{ \begin{array}{l} \bar{w} < \tilde{w} \Rightarrow \left\{ \bar{r}_f = \bar{r}_i(\bar{w}); \frac{\partial \bar{r}_i(\bar{w})}{\partial \bar{w}} = 0 \right\} \\ \bar{w} = \tilde{w} \Rightarrow \left\{ \bar{r}_f = \bar{r}_i(\bar{w}); \frac{\partial \bar{r}_i(\bar{w})}{\partial \bar{w}} > 0 \right\} \\ \tilde{w} < \bar{w} < w_f^{max} \Rightarrow \left\{ \bar{r}_f < \bar{r}_i(\bar{w}); \frac{\partial \bar{r}_i(\bar{w})}{\partial \bar{w}} > 0 \right\} \\ w_f^{max} \geq \bar{w} \Rightarrow \left\{ \bar{r}_i(\bar{w}) = \bar{r}_i^{max}; \frac{\partial \bar{r}_i(\bar{w})}{\partial \bar{w}} = 0 \right\} \end{array} \right. \quad (3.33)$$

where \bar{r}_i^{max} is the average revenue when the zero cutoff profit condition is given by Γ_i^{max} .

Using Proposition 3.7.1, the proof of Proposition 3.3.2.A is straightforward so we focus on Proposition 3.3.2.B. We first derive that the price charged by firms at the productivity cutoff increases with the wage floor. Consequently, as the revenue of firms at the productivity cutoff is constant and equal to σf , this directly implies that the revenues of firms paying a wage higher than the wage floor increase with the wage floor.

Price charged by the firms at the productivity cutoff First, we prove that the price charged by the firms at the productivity cutoff is an increasing function of the wage floor. At the equilibrium situation, both the free entry condition and the production cutoff condition hold. Then we derive

$$\frac{\partial [f(\Gamma_i(\phi^*, \bar{w}) - 1)]}{\partial \bar{w}} = \frac{\partial \left[\frac{f_e}{1-G(\phi^*)} \right]}{\partial \phi^*} \frac{\partial \phi^*}{\partial \bar{w}}$$

It can be derived that

$$\begin{aligned} \frac{\partial [f(\Gamma_i(\phi^*, \bar{w}) - 1)]}{\partial \bar{w}} = & \left[\underbrace{\frac{fg(\phi^*)}{[1-G(\phi^*)]^2} \int_{\phi^*}^{\infty} \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma} - \frac{fg(\phi^*)}{1-G(\phi^*)}}_{= \frac{g(\phi^*)}{1-G(\phi^*)} (f\Gamma_i(\phi^*, \bar{w}) - 1)} - \frac{f}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \phi^*} g(\phi) d\phi} \right] \left[\frac{\partial \phi^*}{\partial \bar{w}} \right] + \\ & \frac{f}{1-G(\phi_i^*)} \int_{\phi^*}^{\infty} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \bar{w}} g(\phi) d\phi \end{aligned}$$

Therefore, using that $\frac{\partial \left[\frac{f_e}{1-G(\phi^*)} \right]}{\partial \phi^*} = \frac{\bar{\pi}_i g(\phi^*)}{1-G(\phi^*)} = \frac{g(\phi^*)}{1-G(\phi^*)} (f\Gamma_i(\phi^*, \bar{w}) - 1)$ and the previous equation implies that

$$\begin{aligned} & \left| \frac{f}{1-G(\phi_i^*)} \int_{\phi^*}^{\infty} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \bar{w}} g(\phi) d\phi \right| = \\ & \left| \left[\frac{f}{1-G(\phi_i^*)} \int_{\phi^*}^{\infty} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \phi^*} g(\phi) d\phi \right] \left[\frac{\partial \phi^*}{\partial \bar{w}} \right] \right| \end{aligned}$$

Furthermore, it can be derived that

$$\begin{aligned} & \left| \frac{f}{1-G(\phi_i^*)} \int_{\phi^*}^{\infty} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \bar{w}} g(\phi) d\phi \right| = \\ & \left| \frac{f}{1-G(\phi_i^*)} \left[\underbrace{\int_{\phi^*}^{\tilde{\phi}(\phi^*, \bar{w})} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \bar{w}} g(\phi) d\phi}_{=0} + \int_{\tilde{\phi}(\phi^*, \bar{w})}^{\infty} \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma} \frac{\sigma-1}{\bar{w}} g(\phi) d\phi \right] \right| \end{aligned}$$

And therefore

$$\begin{aligned} & \left| \frac{f}{1-G(\phi_i^*)} \int_{\phi^*}^{\infty} \frac{\partial \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma}}{\partial \phi^*} g(\phi) d\phi \right| = \\ & \left| \frac{f}{1-G(\phi_i^*)} \int_0^{\infty} \left[\frac{w_i(\phi, \bar{w}) \phi^*}{\bar{w} \phi} \right]^{1-\sigma} \frac{\sigma-1}{\phi^*} g(\phi) d\phi \right| \end{aligned}$$

The previous equations, combined with the fact a strictly positive number of firm pay a wage equal to the wage floor, directly implies that

$$\left| \frac{\partial \phi^*}{\partial \bar{w}} \frac{\bar{w}}{\phi^*} \right| < 1 \quad (3.34)$$

ie

$$\left| \frac{\partial \phi^*}{\phi^*} \right| < \left| \frac{\partial \bar{w}}{\bar{w}} \right| \quad (3.35)$$

Recall that equation 3.7 states that $p(\phi, w) = \frac{w}{\rho\phi}$. Therefore

$$\frac{dp(\phi^*(\bar{w}), w)}{d\bar{w}} = \frac{1}{\rho\phi^*(\bar{w})^2} \left(\phi^*(\bar{w}) - \frac{\partial \phi^*(\bar{w})}{\bar{w}} \bar{w} \right)$$

Equation 3.35 then implies that $\frac{dp(\phi^*(\bar{w}), w)}{d\bar{w}} > 0$. In other words, the price charged by the firms at the productivity cutoff is an increasing function of the wage floor.

Revenues of firms paying a wage higher than the wage floor increase with the wage floor

Using the fact that the revenue of the firm at the productivity cutoff is a constant, we can derive that

$$\frac{r_i(\phi, \bar{w})}{r_f(\phi)} = \left[\frac{\bar{w}}{\tilde{w}} \cdot \frac{\phi_f^*}{\phi_i^*(\bar{w})} \cdot \frac{w_f(\phi)}{w_i(\phi, \bar{w})} \right]^{\sigma-1}$$

Using this equation it is straightforward to establish that as long as $w_i(\phi) > \bar{w}$ the revenue and the wage paid by a firm of productivity ϕ are increasing functions of the wage floor. Figure 3.3.3 depicts both the situation where there is no wage floor and the one where there is a wage floor respecting the following condition $\tilde{w} < \bar{w} < w_f^{max}$.

Value of the wage floor

Proof of Proposition 3.3.3.A

Proof that $\bar{w} > \tilde{w}$

If the wage floor was equal to the reservation wage, then the composition of the industry would be the same than the one with the decentralized bargaining. Therefore, the derivative of the industry bargaining problem evaluated at $\bar{w} = \tilde{w}$ is given by

$$\frac{\beta \left[\int_{\phi^*}^{\infty} \frac{\partial[(w(\phi, \bar{w}) - \tilde{w})l(\phi, \bar{w})]}{\partial \bar{w}} g(\phi) d\phi - \underbrace{(w(\phi^*, \bar{w}) - \tilde{w})l(\phi^*, \bar{w})}_{=0} g(\phi^*) \frac{\partial \phi^*}{\partial \bar{w}} \right]}{\int_{\phi^*}^{\infty} [(w(\phi, \bar{w}) - \tilde{w})l(\phi, \bar{w})] g(\phi) d\phi} + \frac{(1 - \beta) \left[\int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi - \underbrace{\pi(\phi^*, \bar{w})}_{=0} g(\phi^*) \frac{\partial \phi^*}{\partial \bar{w}} \right]}{\int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g(\phi) d\phi}$$

Using previous results, the wage floor increase will raise the productivity cutoff, and the wages paid by every remaining firms. This will have a strictly positive effect on the industry aggregate price P_j , which directly implies that for any firm, when $\bar{w} = \tilde{w}$

$$\frac{1}{(w_i(\phi, \bar{w}) - \tilde{w})l_i(\phi, \bar{w})} \frac{\partial(w(\phi, \bar{w}) - \tilde{w})l_i(\phi, \bar{w})}{\partial \bar{w}} > \frac{1}{(w_f(\phi) - \tilde{w})l_f(\phi)} \frac{\partial(w_f(\phi) - \tilde{w})l_f(\phi)}{\partial w} \quad (3.36)$$

and

$$\frac{1}{\pi_i(\phi, \bar{w})} \frac{\partial \pi_i(\phi, \bar{w})}{\partial \bar{w}} > \frac{1}{\pi_f(\phi)} \frac{\partial \pi_f(\phi)}{\partial w} \quad (3.37)$$

Recall that the firm-level bargaining solution solves:

$$\max_w \left\{ [(w - \tilde{w}) l_f(\phi, w)]^\beta [\pi_f(\phi, w)]^{1-\beta} \right\} \quad (3.38)$$

ie :

$$\beta \frac{1}{(w_f(\phi) - \tilde{w}) l_f(\phi)} \frac{\partial (w_f(\phi) - \tilde{w}) l_f(\phi)}{\partial w} + (1 - \beta) \frac{1}{\pi_f(\phi)} \frac{\partial \pi_f(\phi)}{\partial w} = 0 \quad (3.39)$$

Equations 3.36 and 3.37 lead to :

$$\beta \frac{1}{(w_i(\phi) - \tilde{w}) l_i(\phi)} \frac{\partial (w_i(\phi) - \tilde{w}) l_i(\phi)}{\partial w} + (1 - \beta) \frac{1}{\pi_i(\phi)} \frac{\partial \pi_i(\phi)}{\partial w} > \quad (3.40)$$

$$\beta \frac{1}{(w_f(\phi) - \tilde{w}) l_f(\phi)} \frac{\partial (w_f(\phi) - \tilde{w}) l_f(\phi)}{\partial w} + (1 - \beta) \frac{1}{\pi_f(\phi)} \frac{\partial \pi_f(\phi)}{\partial w} = 0 \quad (3.41)$$

Therefore, when the wage floor is equal to the reservation wage, the derivative of the industry-level bargaining problem is strictly positive. The reservation wage is therefore not a solution of industry-level bargaining maximization problem. Consequently, $\bar{w} > \tilde{w}$.

Proof that the wage floor equation has a finite solution

If the wage floor is high enough to be binding for every firm operating in the economy, the bargaining problem becomes

$$\beta \left[\frac{-\xi}{\bar{w}} + \frac{1}{(\bar{w} - \tilde{w})} \right] + \frac{(1 - \beta)(1 - \xi) \bar{r}^{max}}{\bar{w} (\bar{r}^{max} - \sigma f)} = 0$$

The previous equation has a finite solution.

Proof of Proposition 3.3.3.B

Results of Proposition 3.3.2 and of Proposition 3.3.3.A directly imply Proposition 3.3.3.B

3.7.5 Appendix E: General Equilibrium

Proof of Proposition 3.3.4

The price equation implies that the national aggregate revenue is constant equal to E . Moreover, E is equal to

$$E = \frac{1}{1 - G(\phi_k^*)} \int_{\phi_k^*}^{\infty} w_k(\phi) \frac{q_k(\phi)}{\phi} g(\phi) d\phi M_k$$

Where $k \in \{f, i\}$. The employment level is given by

$$\frac{1}{1 - G(\phi_k^*)} \int_{\phi_k^*}^{\infty} \frac{q_k(\phi)}{\phi} g(\phi) d\phi M_k$$

When there is a wage floor the wage of every firm and the productivity cutoff increase. Consequently, as wages are always increasing in productivity, it can be derived that for any given level of employment:

$$\frac{1}{1 - G(\phi_k^*)} \int_{\phi_k^*}^{\infty} w_k(\phi) \frac{q_k(\phi)}{\phi} g(\phi) d\phi M_k$$

is higher when $k = i$ than when $k = f$. This directly implies that the level of employment decreases under industry-level bargaining.

Furthermore, when the wage floor increases, the average revenue increases. Therefore, equation 3.26 and the condition on the numeraire imply that the mass of firms decreases and, as the productivity cutoff increases, the diversity of products decreases. Finally, the price of remaining products increases. Building on previous insights, we conclude that the utility of the consumer decreases.

3.7.6 Appendix F: Impact of the representativeness of bargaining institutions

Micro-foundation for the existence of employers federations dominated by large firms

In this section, we micro-found the fact that employers federations are dominated by large firms - ie attribute higher weights to larger firms. We sketch a model in which firms compete, through lobbying activities, to ensure that the objective of the employers federation is as close as possible to their objective. Our assumptions are very similar to the standard assumptions of the political literature focusing on lobby and competition for representation (see Persson and Tabellini (2002)). We find that the more productive a firm is, the higher its investment in lobbying activities, and therefore the higher its weight in the employers federation's objective.

Timing of events

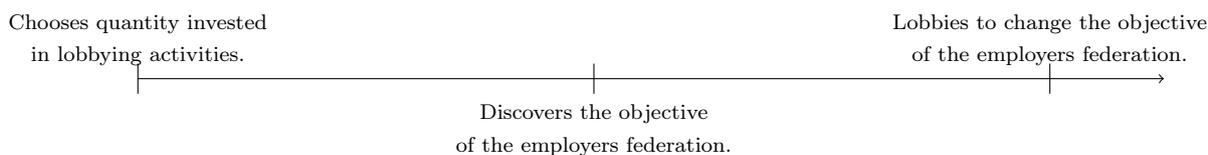


Figure 3.7.2 – Timeline of an entrepreneur

We suppose that lobbying activities occur before the wage bargaining. Entrepreneurs observe the quantity invested by others, but, following the literature on political economics (see Persson and Tabellini (2002), Grossman and Helpman (1996), Baron (1994)), we assume that entrepreneurs face uncertainty about the preferences of the representative agent. For simplicity, we suppose that they have complete uncertainty. Formally, a firm expects the wage floor negotiated by the employers federation to be randomly distributed over $[\tilde{w}; \infty[$. The entrepreneur first chooses the quantity of money to invest in lobbying activities, then she discovers the objective of the employers federation.

Once the entrepreneur has discovered the objective of the industry representative, she uses the money she invested in lobbying activities to ensure that the objective of the latter will be as close as possible to its own (in terms of the previous model, this corresponds to the function $h(\cdot)$). Formally, investments enable firms to change the wage negotiated at the sector level, \bar{w} . We denote the level of investments by I , and we assume that the efficiency of lobbying activities is an increasing function of the firm's investment effort relative to competitors. More precisely, we assume that:

$$\left| \frac{\partial \bar{w}}{\partial I} \right| = z \left(\frac{I}{\bar{I}} \right) \quad (3.42)$$

where \bar{I} is the total amount of investments made by all firms of the sector, and $z(\cdot)$ is an increasing and concave function. The use of $\frac{I}{\bar{I}}$ implies that it is not the level of investment *per se* that matters, but the effort relative to competitors. We assume that this function is independent of both the size and revenues of the firm.

While this assumption is relatively restrictive, it is likely that were the function dependent on the firm's size, it would be increasing in firm's size (see Offerlé (2009) which develops the idea that employers federations encourage the participation of large firms). As a result, taking into account such a larger efficiency of investments for large firms would only strengthen our results.

Finally, we assume that if I is inferior to an exogenous parameter c , the firm invests nothing and is not member of the employers federation. This represents the fixed cost associated with participation to the federation.

Quantity invested in lobbying activities

The objective of the entrepreneurs is given by

$$\begin{aligned} & \max_I \{ \mathbb{E}_{\bar{w}} (\pi(\phi, \bar{w})) - I \} \\ & \text{s.t. } \left| \frac{\partial \bar{w}}{\partial I} \right| = z \left(\frac{I}{\bar{I}} \right) \end{aligned} \quad (3.43)$$

We denote $\hat{w}(\phi)$ the wage negotiated at the firm level, by firms with a level of productivity equal to ϕ , given by equation (3.30). It can be derived that

$$\frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} = \begin{cases} \frac{(\sigma-1)f}{\sigma} \left| \frac{\epsilon_{\bar{w}}^{\phi^*}}{\bar{w}} \right| r(\phi, \bar{w}) & \text{if } \bar{w} < \hat{w}(\phi) \\ \frac{(\sigma-1)f}{\sigma} \left| \frac{\epsilon_{\bar{w}}^{\phi^*}}{\bar{w}} \right| r(\phi, \bar{w}) & \text{if } \bar{w} > \hat{w}(\phi) \end{cases} \quad (3.44)$$

Where ϵ_y^x is the elasticity of y with respect to x . If the objective of the employers federations implies a wage lower than the wage negotiated by the entrepreneur at the firm level (*i.e.* if $\bar{w} < \hat{w}(\phi)$) the entrepreneur lobbies to increase the negotiated wage. The opposite is true if the objective of the employers federations implies a wage higher to the wage negotiated by the entrepreneur at the firm level (*i.e.* if $\bar{w} > \hat{w}(\phi)$).

It must be noted that the incentive is higher when the wage is binding for the firm. Indeed, it means that this firm pays a wage equal to the wage floor. Therefore, when lobbying, the firm will directly decrease its labor cost. However, when the firm negotiates at the firm level a wage higher than the wage floor, it only benefits from the higher labor cost of small competitors.

Moreover, the incentives are proportional to the revenue of a firm. As a consequence, the higher the revenue, the stronger the incentive to engage in lobbying activities.

Equations 3.43 and 3.44 imply that

$$\frac{\sigma}{f(\sigma-1)} \frac{1}{z\left(\frac{I}{I}\right)} = \int_{\bar{w}}^{\hat{w}(\phi)} \frac{|\epsilon_{\bar{w}}^{\phi^*}|}{w} r(\phi, w) dw + \int_{\hat{w}(\phi)}^{\infty} \frac{|\epsilon_w^{\phi^*}|}{w} r(\phi, w) dw \quad (3.45)$$

We compute the evolution of the money invested in lobbying activities when the productivity, or equivalently the size, of the firm increases. It can be derived that

$$\begin{aligned} \frac{\sigma}{f(\sigma-1)} \frac{\partial \left[\frac{\partial(\mathbb{E}_{\bar{w}}\{\pi(\phi, \bar{w})\})}{\partial \bar{w}} \right]}{\partial \phi} &= \int_{\bar{w}}^{\hat{w}(\phi)} \frac{|\epsilon_{\bar{w}}^{\phi^*}|}{w} \frac{\partial r(\phi, w)}{\partial \phi} dw + \frac{\partial \hat{w}(\phi)}{\partial \phi} \frac{|\epsilon_{\hat{w}(\phi)}^{\phi^*}|}{\hat{w}(\phi)} r(\phi, \hat{w}(\phi)) \\ &\quad - \frac{\partial \hat{w}(\phi)}{\partial \phi} \frac{|\epsilon_{\hat{w}(\phi)}^{\phi^*}|}{\hat{w}(\phi)} r(\phi, \hat{w}(\phi)) + \int_{\hat{w}(\phi)}^{\infty} \frac{|\epsilon_w^{\phi^*}|}{w} \frac{\partial r(\phi, w)}{\partial \phi} dw \end{aligned} \quad (3.46)$$

The first term is positive. It states that the higher the firms revenue, the higher its incentive to invest in lobbying activities. Moreover, the efficiency of investments is independent of the firms size ($z(\cdot)$ is independent of ϕ). Therefore, as large firms generate higher revenues, a given level of investment represents a lower share of their revenue. This effect also drives up investments made by large firms. The same line of reasoning applies to the last term.

The difference between the second and the third term embodies the fact that, as described previously, that the incentive is higher when the wage is binding for the firm.

The only negative factor of this equation is $-\frac{\partial \hat{w}(\phi)}{\partial \phi} \frac{|\epsilon_{\hat{w}(\phi)}^{\phi^*}|}{\hat{w}(\phi)} r(\phi, \hat{w}(\phi))$. Using equation 3.30, it can easily be derived that $\frac{\partial \hat{w}(\phi)}{\partial \phi} \frac{\phi}{\hat{w}(\phi)} < \frac{\partial r(\phi, \hat{w}(\phi))}{\partial \phi} \frac{\phi}{r(\phi, \hat{w}(\phi))}$. Then, it is straightforward to establish that $\frac{\sigma}{f(1-\sigma)} \frac{\partial \left[\frac{\partial(\mathbb{E}_{\bar{w}}\{\pi(\phi, \bar{w})\})}{\partial \bar{w}} \right]}{\partial \phi} > 0$

Proof of Proposition 3.4.1.A

In the following, we compare the wage floor under an unrepresentative employers federation, ie the solution of:

$$\max_{\bar{w}} \left\{ \left[\frac{1}{1 - G(\phi^*(\bar{w}))} \int_{\phi^*}^{\infty} (w(\phi, \bar{w}) - \tilde{w}) l_f(\phi, \bar{w}) Mg(\phi) d\phi \right]^{\beta} [V_h(\bar{w})]^{1-\beta} \right\} \quad (3.47)$$

with the wage floor under a representative employers federation, ie the solution of :

$$\max_{\bar{w}} \left\{ \left[\frac{1}{1 - G(\phi^*(\bar{w}))} \int_{\phi^*}^{\infty} (w(\phi, \bar{w}) - \tilde{w}) l_f(\phi, \bar{w}) Mg(\phi) d\phi \right]^{\beta} [V_i(\bar{w})]^{1-\beta} \right\} \quad (3.48)$$

We thus compare

$$\frac{\frac{1}{1-G_h(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g_h(\phi) d\phi}{\frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g_h(\phi) d\phi}$$

and

$$\frac{\frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi}{\frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g(\phi) d\phi}$$

First we focus on the difference of the denominators, ie:

$$\frac{1}{1-G_h(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g_h(\phi) d\phi - \frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g(\phi) d\phi$$

Following the properties of $h(\cdot)$, given in definition 2, we must have

$$a \in [\phi^*, \infty[, \begin{cases} x \leq a \Rightarrow \frac{g_h(x)}{1-G_h(\phi^*)} \leq \frac{g(x)}{1-G(\phi^*)} \\ x \geq a \Rightarrow \frac{g_h(x)}{1-G_h(\phi^*)} \geq \frac{g(x)}{1-G(\phi^*)} \end{cases}$$

Therefore, as profit is an increasing function of productivity, we have

$$\begin{aligned} & \frac{1}{1-G_h(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g_h(\phi) d\phi - \frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g(\phi) d\phi \geq \\ & \pi(a, \bar{w}) \int_{\phi^*}^a \left[\frac{g_h(x)}{1-G_h(\phi^*)} - \frac{g(x)}{1-G(\phi^*)} \right] d\phi + \pi(a, \bar{w}) \int_a^{\infty} \left[\frac{g_h(x)}{1-G_h(\phi^*)} - \frac{g(x)}{1-G(\phi^*)} \right] d\phi = 0 \end{aligned}$$

This implies that for every wage floor level,

$$\frac{1}{1-G_h(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g_h(\phi) d\phi \geq \frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g(\phi) d\phi$$

Then, we focus on the difference of the numerators:

$$\frac{1}{1-G_h(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g_h(\phi) d\phi - \frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi$$

We define c as the productivity level such that $\frac{\partial \pi(c, \bar{w})}{\partial \bar{w}} = \max_{x \in [\phi^*, a]} \left\{ \frac{\partial \pi(x, \bar{w})}{\partial \bar{w}} \right\}$ and d such as the productivity level such that $\frac{\partial \pi(d, \bar{w})}{\partial \bar{w}} = \min_{x \in [a, \infty]} \left\{ \frac{\partial \pi(x, \bar{w})}{\partial \bar{w}} \right\}$.

Using those definitions, we have

$$\begin{aligned} & \frac{1}{1-G_h(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g_h(\phi) d\phi - \frac{1}{1-G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi \geq \\ & \frac{\partial \pi(c, \bar{w})}{\partial \bar{w}} \int_{\phi^*}^a \left[\frac{g_h(x)}{1-G_h(\phi^*)} - \frac{g(x)}{1-G(\phi^*)} \right] d\phi + \frac{\partial \pi(d, \bar{w})}{\partial \bar{w}} \int_a^{\infty} \left[\frac{g_h(x)}{1-G_h(\phi^*)} - \frac{g(x)}{1-G(\phi^*)} \right] d\phi \geq \end{aligned}$$

$$\frac{\partial \pi(c, \bar{w})}{\partial \bar{w}} \int_{\phi^*}^{\infty} \left[\frac{g_h(x)}{1 - G_h(\phi^*)} - \frac{g(x)}{1 - G(\phi^*)} \right] d\phi = 0$$

This implies that for every wage floor level,

$$\frac{1}{1 - G_h(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g_h(\phi) d\phi \geq \frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi$$

Finally, as already demonstrated $\frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi$ is negative. Therefore:

$$\frac{\frac{1}{1 - G_h(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g_h(\phi) d\phi}{\frac{1}{1 - G_h(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g_h(\phi) d\phi} \geq \frac{\frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \frac{\partial \pi(\phi, \bar{w})}{\partial \bar{w}} g(\phi) d\phi}{\frac{1}{1 - G(\phi^*)} \int_{\phi^*}^{\infty} \pi(\phi, \bar{w}) g(\phi) d\phi}$$

Therefore, for all function respecting the condition of definition 2, the negotiated wage floor is higher than in the "equal weights" scenario.

Using definition 3, the proof of Proposition 3.4.1.B is derived using the same method as previously.

3.7.7 Appendix G: Descriptive statistics

Table A1 – Industry agreements most dominated by large firms

Industry i	Industry label	Percentile of bargaining firm size within industry distribution	Bargaining firms x times larger than average firm	% of firms which bargain
0440	Temporary work	1.00	6.14	0.40
2148	Telecommunications	1.00	46.76	0.04
2060	Canteens and related (chains)	1.00	34.13	0.03
2198	E-commerce firms	1.00	32.70	0.02
1225	Departmental businesses of La Reunion	1.00	9.11	0.01
2098	Service providers in tertiary sector	0.99	25.34	0.03
1618	Camping industry	0.99	19.66	0.02
0086	Advertising and related	0.99	12.85	0.02
2156	Department and variety stores	0.99	19.63	0.06
2412	Cartoons production	0.99	8.95	0.12

Table A2 – Industry agreements least dominated by large firms

Industry i	Industry label	Percentile of bargaining firm size within industry distribution	Bargaining firms x times larger than average firm	% of firms which bargain
1875	Veterinary offices and cliniques	0.18	0.25	0.47
1631	Outdoor hotel business	0.32	0.44	0.85
1978	Florists	0.44	0.61	0.63
3168	Photography industry	0.49	0.44	0.44
2704	Guadeloupe, St-Martin and St-Barthelemy banks	0.50	1.09	1.00
2701	Guyane banks	0.50	1.27	0.50
1182	Marinas staff	0.52	0.46	0.69
2335	Insurance companies staff	0.55	0.63	0.96
1619	Dental practices	0.56	0.62	0.80
1671	Students homes	0.58	0.66	0.36

3.7.8 Appendix H: OLS results

Table A3 – The role of representativeness on wage floor increase (in %) - OLS results

	Wage floor increase (%)				
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS
$S_{ijt} \times \mathbb{1}(U_j > \bar{U}_j)$	0.523*** (0.0897)	0.428*** (0.0887)	0.425*** (0.0891)	0.428*** (0.0891)	0.423*** (0.0894)
$S_{ijt} \times \mathbb{1}(U_j < \bar{U}_j)$	0.446*** (0.0948)	0.345*** (0.0905)	0.341*** (0.0916)	0.349*** (0.0955)	0.371*** (0.101)
ΔS_{ijt-1}				-0.00690 (0.0.00745)	-0.00709 (0.00801)
ΔS_{ijt-2}					0.00319 (0.00565)
# obs	9804	9800	9798	9664	9543
Adj R2	0.87	0.87	0.87	0.87	0.87
Occupation FE	No	Yes	No	No	No
Occupation \times Year FE	No	No	Yes	Yes	Yes
Industry \times Year FE	Yes	Yes	Yes	Yes	Yes

Note: The dependent variable is the wage floor variation between $t - 1$ and t . S_{ijt} denotes the share of workers employed in small firms for the skill-level position i , industry j and year t . $\mathbb{1}(U_j > \bar{U}_j)$ is a dummy equal to one when the industry j is less representative than the median industry: U_j is a measure of unrepresentativeness (the larger U_j , the higher the domination of the employers federations by large firms), and \bar{U}_j is the median unrepresentativeness. Standard errors are clustered at the industry \times year level. Standard errors are given in brackets. *, **, and *** denote statistical significance at 10, 5 and 1%.

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Titre : Croissance des entreprises et marchés frictionnels

Mots clés : dynamiques d'entreprises, commerce international, économie du travail, réseaux.

Résumé : Cette thèse s'inscrit dans les domaines de l'économie internationale et de l'économie du travail, et porte sur la croissance des entreprises. Les dynamiques d'entreprises jouent un rôle clé dans la détermination des résultats agrégés. Il est donc essentiel de comprendre comment les entreprises se développent pour comprendre la croissance économique, l'innovation et le fonctionnement du marché du travail. Les frictions, tant sur le marché des biens que sur le marché du travail, constituent l'un des facteurs ralentissant la croissance des entreprises. D'une part, les frictions informationnelles et contractuelles entre vendeurs et acheteurs empêchent les entreprises d'acquérir de nouveaux acheteurs. D'autre part, les frictions sur le marché du travail, en augmentant les coûts d'embauche et de licenciement des travailleurs, limitent la réallocation efficace des travailleurs entre les entreprises, ce qui ralentit la croissance des entreprises sur les marchés

nationaux et étrangers. Cette thèse analyse l'impact de ces deux types de frictions sur la croissance et la survie des entreprises.

Le premier chapitre identifie l'impact causal du débauchage de commerciaux provenant d'entreprises rivales sur la capacité d'une entreprise à développer son portefeuille de clients à l'international. Les deux derniers chapitres portent sur l'effet des frictions du marché du travail, et plus particulièrement sur l'effet des politiques du marché du travail, sur la croissance et la survie des entreprises. Le deuxième chapitre décrit les biais des juges dans les tribunaux du travail et comment ils peuvent affecter les perspectives de croissance des entreprises poursuivies. Le troisième chapitre traite des accords de branche et met en avant un mécanisme par lequel les grandes entreprises peuvent utiliser la négociation sectorielle comme un dispositif anticoncurrentiel.

Title: Firms' growth in frictional markets

Keywords: firm dynamics, international trade, labor economics, networks.

Abstract: This thesis lies in the fields of International Trade and Labor Economics and focuses on firms' growth. Firm dynamics play a key role in shaping aggregate outcomes. Understanding how firms grow and how they decline is thus crucial to understand aggregate growth, innovation, and the functioning of labor markets. One of the widely-recognized factors slowing down firms' growth is frictions, both on the product market and the labor market. On the one hand, information and contractual frictions between sellers and buyers preclude firms from reaching new buyers. On the other hand, labor market frictions, by increasing the costs to hire and dismiss workers, reduce the efficient reallocation of workers across firms, thereby slowing down firms' growth on domestic and

foreign markets. This thesis analyzes the impact of those two types of frictions on firms' growth and survival.

The first chapter identifies a causal impact of poaching a sales manager from other firms' labor force on a firm's ability to develop its international portfolio of clients. The two remaining chapters consider the effect of labor market frictions, and more specifically labor market policies, on firms' growth and survival. The second chapter analyses judge bias in labor courts and how it can affect the sued firms' growth prospects. The third chapter deals with sectoral agreements and highlights a mechanism by which large firms can use sectoral bargaining as an anti-competitive device.